





# RADFORD LIBRARY,

Saint Mary's Hospital, Manchester.

No. *P2*

*This Book to be returned in* \_\_\_\_\_ *days.*

*Fine for overtime* \_\_\_\_\_ *per day.*

---

*Note*—No book can be renewed if wanted by another reader, nor unless brought to the Library for that purpose.

It is requested that the leaves of books may not be turned down,—that no person will write in them,—and that the greatest possible care may be taken of them.

---

## EXTRACTS FROM THE RULES.

That each Medical Officer shall be allowed not more than two works out of the Library at one time, and not more than two volumes of each work.

That Registered Medical Students shall be allowed to take out books every Tuesday and Saturday, from eleven till one, or at such hours as may be ordered from time to time by the Board.

That each Registered Medical Student shall be allowed to have not more than one book out of the Library at the same time, unless the work consists of two or more volumes, and in no case more than two volumes.

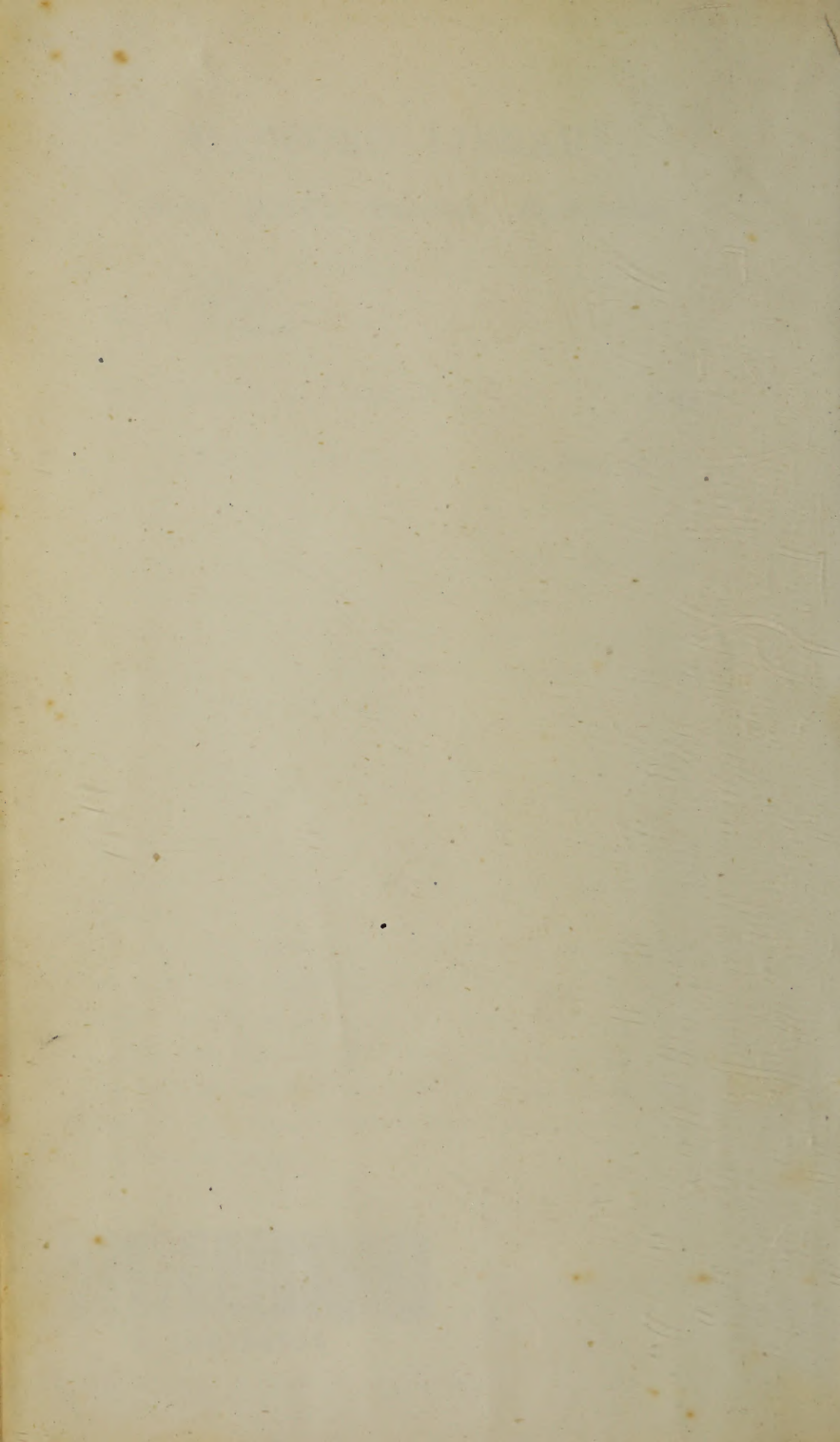


22102356534



Dr. R. A. R. R. R.  
312













THE  
DUBLIN JOURNAL

OF

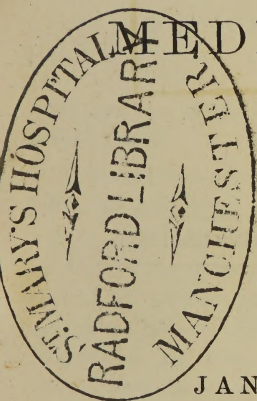
MEDICAL SCIENCE.

---

VOL. LVII.

JANUARY TO JUNE, 1874.

---



DUBLIN:  
FANNIN AND COMPANY, GRAFTON-STREET.  
LONDON: LONGMANS & CO.; SIMPKIN, MARSHALL & CO.  
EDINBURGH: MACLACHLAN AND STEWART.  
PARIS: HACHETTE & CO.

---

1874.



DUBLIN: JOHN FALCONER, PRINTER, 53, UPPER SACKVILLE-STREET.



WELLCOME INSTITUTE LIBRARY	
Coll.	well 40mec
Call	ser
No.	W1
	10121

## NOTICES TO CORRESPONDENTS.

---

We have been obliged to hold over several Original Communications, Reviews, and Clinical Records.

Authors of Communications are requested to write the prescriptions in their paper, in full, and in English.

Books and Periodicals published in Northern Europe and the German States intended for our Journal, should be transmitted "For the Editor of the Dublin Journal of Medical Science, care of Messrs. TRUBNER and Co., *London*," through the following Houses, viz.:—Messrs. SCHNEIDER and Co., *Berlin*; Messrs. BRAUMULLER and SON, *Vienna*; M. F. A. BROCKHAUS, *Leipzig*; and also their Correspondents in the principal Towns on the Continent. Our Correspondents in France, Belgium, Italy, and Spain, are requested to communicate with us through "Messrs. HACHETTE and Co., 77, *Boulevard St. Germain, Paris*."

AMERICAN Books and Journals often come to hand with such an amount of Charges on them, that we cannot release them. It is requested that *all* communications from the United States shall be forwarded to MR. JOHN WILEY, *New York*; or MR. HENRY C. LEA, *Philadelphia*, directed to us, to the care of Messrs. TRUBNER and Co., *London*.





# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

### CONTENTS.

THIRD SERIES, No. XXV.—JANUARY 1, 1874.

#### PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. I.—Lessons from General Practice. By SAMUEL T. KNAGGS, M.D., Ch.M., F.R.C.S.I., L.K.Q.C.P.I.; Honorary Surgeon, Newcastle Hospital, New South Wales; Assistant Surgeon, New South Wales Naval Forces—(Illustrated):—	
Cases of Fracture of the Femur Successfully Treated by an Apparatus with Weight and Pulley, - - - -	1
ART. II.—Report on Small-pox in 1871, '72, and '73, at the Workhouse Hospital, Mullingar; with some Remarks on Treatment. By DILLON KELLY, M.R.C.S.E.; Medical Officer to the Hospital, - - -	6
ART. III.—Practical Observations. By ROBT. FITZMAURICE, L.K.Q.C.P.I., L.R.C.S.I., &c.; Physician to the County Kerry Fever Hospital:—	
I.—Remarks on Small-pox, - - - - -	13
II.—Notes of a Case of Empyema, in which several Aspirations were Performed—Fatal Issue, - - - - -	15

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative. By SAMUEL D. GROSS, M.D., LL.D., D.C.L., Oxon.; Professor of Surgery in the Jefferson Medical College of Philadel- phia, &c., &c., - - - - -	20
2. Insanity, and Hospitals for the Insane in Ireland. Twenty-second Report of the District, &c., Asylums in Ireland, 1872. Presented to both Houses of Parliament, - - - - -	44



	PAGE
3. Maclachlan and Stewart's Pathological Charts. No. 1. The Brain, -	48
4. A Treatise on the Continued Fevers of Great Britain. By CHARLES MURCHISON, M.D., LL.D., F.R.S., - - - - -	48
5. Recent Works on the Diseases of the Urinary Organs. By Sir HENRY THOMPSON :—	
I.—Clinical Lectures on Diseases of the Urinary Organs, -	56
II.—The Preventive Treatment of Calculous Disease and the Use of Solvent Remedies, - - - - -	56

### PART III.—MEDICAL MISCELLANY.

#### Transactions of the Medical Society of the College of Physicians :—

Therapeutical Remedies recently Introduced. By WALTER GEORGE SMITH, M.D., Fellow and Censor, K.Q.C.P.I., - - -	57
Further Observations on the Use of Hemlock. By HENRY KENNEDY, V.P. and Censor, College of Physicians, - - -	67

#### Proceedings of the Dublin Obstetrical Society :—

The Preventive Treatment of Post-partum Hæmorrhage. By A. H. M'CLINTOCK, M.D., &c., &c., - - - - -	75
--	----

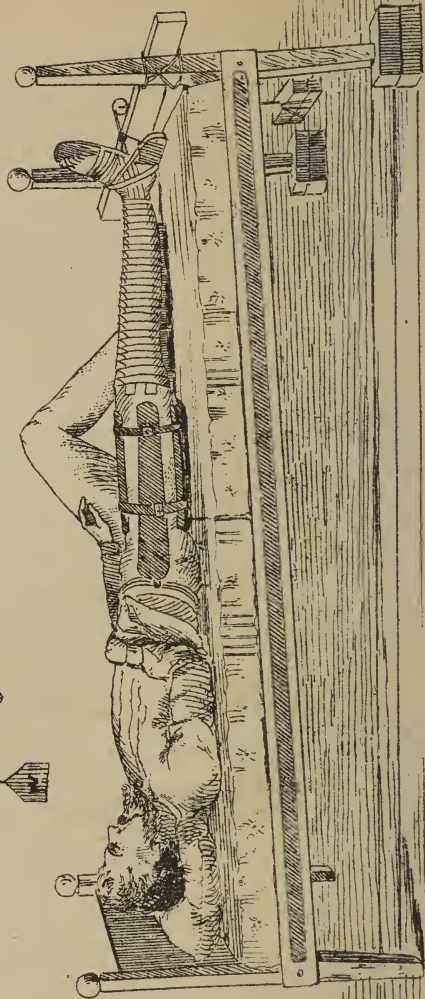
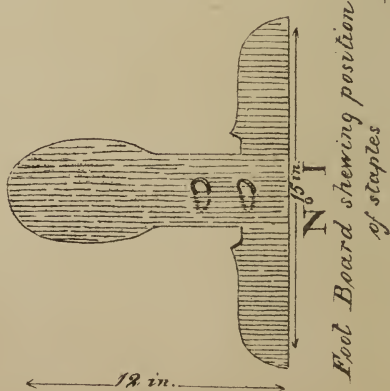
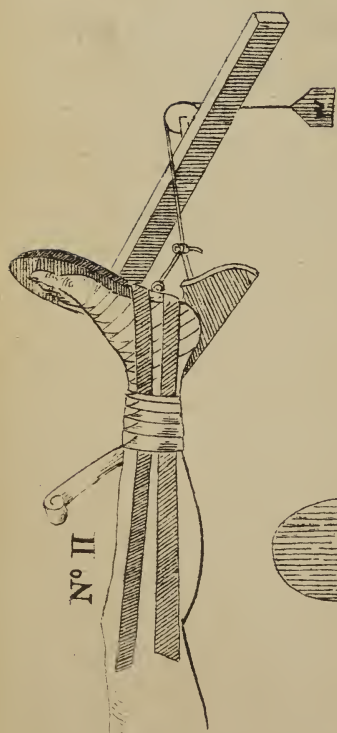
#### Proceedings of the Pathological Society of Dublin :—

DR. BARTON on Medullary Cancer of Clavicle, with Secondary Dissemination, - - - - -	92
MR. PORTER on Cystic Disease of the Ovary, - - - - -	94
MR. PORTER on Fracture of Os Calcis, - - - - -	94
DR. BARTON on Osteitis, - - - - -	96

Books Received, - - - - -	- Cover.
---------------------------	----------







EXTENSION APPARATUS FOR FRACTURE OF THE FEMUR BY MEANS OF WEIGHT AND PULLEY.

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

JANUARY 1, 1874.

---

### PART I.

### ORIGINAL COMMUNICATIONS.

---

ART. I.—*Lessons from General Practice.* By SAMUEL T. KNAGGS, M.D., Ch.M., F.R.C.S.I., L.K.Q.C.P.I; Honorary Surgeon, Newcastle Hospital, New South Wales; Assistant Surgeon, New South Wales Naval Forces.

#### CASES OF FRACTURE OF THE FEMUR SUCCESSFULLY TREATED BY AN EXTENSION APPARATUS WITH WEIGHT AND PULLEY.

IN this Journal for November, 1871, Mr. B. W. Richardson, of Dublin, contributes a valuable article on "Fracture of the Thighbone, and its Treatment with the American Apparatus." I had the advantage of examining and carefully measuring one of the cases reported by Mr. Richardson, and the satisfactory result exhibited impressed me most favourably with the method of treatment then adopted by him.

I now wish to place on record two cases of fracture of the femur, treated on an identical principle, though carried into effect by what I believe to be a more simple method.

The apparatus now about to be described has been for some time in use at the Newcastle Hospital, New South Wales. It is a slight modification of that described by Dr. Gurdon-Buck,<sup>a</sup> of New York. The following constitute the apparatus employed:—

<sup>a</sup> Description of an Improved Extension Apparatus, for the Treatment of Fracture of the Thigh, &c., introduced by Gurdon-Buck, M.D., Surgeon to the New York Hospital, St. Luke's Hospital, &c., &c.

(1.) Four slips of adhesive plaster, two inches wide, and from thirty to thirty-six inches long.

(2.) Two bandages, six feet long, by two and a-half inches wide.

(3.) A foot-board, such as is delineated (Fig. 1) in sketch; it is made out of deal, one inch thickness, in the proportion shown in the drawing, and fitted with two staples, for the attachment of the rope by which extension is made.

(4.) A piece of one and a-half inch board, four feet long by five inches wide, fitted with a common iron pulley.

(5.) A set of three splints, properly padded, and fitted with straps and buckles.

(6.) Two yards of rope, such as is used for clothes lines, or suspending window sashes, and a few yards of strong twine.

(7.) Six bricks.

With an ordinary four-post bedstead, palliasse, hair mattress, and a pair of pillows, the appliances are complete.

The patient is placed on his back on the hair mattress, though this is not indispensable, as I placed the patient, in Case II., on a straw palliasse, with a pillow under his buttocks. The limb is sponged with vinegar diluted with water, and carefully dried. The four strips of plaster are heated, and applied to the leg, two at each side, extending from a short distance above the knee, and carried down the leg, the free ends to hang eight or ten inches below the sole of the foot. This strapping is further secured to the limb by a carefully applied bandage from the toes upwards. In practice it will be found best not to include the strapping in the bandage until above the ankle, as in Fig. 2. The foot-board (Fig. 1) is applied to the sole of the foot, and is firmly secured there, like a sandal, by tying the free ends of the straps of plaster beneath it. The posterior straps are to be tied between the two staples, and the anterior ones above the upper staple. The foot is then securely bandaged to the foot-board by a figure of 8 bandage, care being taken to leave the loops of the staple free. This bandage has not yet been applied in Fig. 2. The board fitted with the pulley is adjusted to the foot of the bed by being tied to the posts with the stout twine, care being taken that the pulley be at a proper height, and in a line with the uppermost staple. The sash line is secured to the two staples by one end, in such a manner as to make equable traction on the foot-board, and two bricks are attached to the other end; this weight (or an equivalent of fourteen pounds) forms the means of extension.

The foot of the bed is raised about five inches, by placing two bricks under each leg; this gives the bed a descending incline towards the head, so that the gravitation of the patient's body forms the means of counter-extension. The sound and injured limbs are carefully measured, and any shortening of the latter may be remedied by gradual forcible extension and counter-extension. The thigh splints are applied, and the adaptation of the apparatus is rendered complete by placing a small flat feather pillow beneath the calf and heel of the injured limb, to obviate such results as would accrue from their continued pressure on the hard mattress or palliasse.

A brief study of the accompanying drawing will, I hope, simplify that which, perhaps, the elaborate description may have apparently rendered formidable.

CASE I.—F. B., an able-bodied seaman, aged thirty years, employed on board the schooner "Sir Isaac Newton." He got his thigh interposed between the schooner and another vessel during a collision in a heavy sea off the Port of Newcastle, on 8th July, 1872. The next day I saw him, and found that he had sustained a comminuted fracture at the lower third of the femur; the soft parts of the thigh and knee were terribly crushed. I put the limb up temporarily, and had him strapped to a plank, in order to facilitate his transit through the small hatchway of the schooner, and conveyance by boat to the wharf, thence to the hospital, where he was admitted under the care of Mr. R. Harris. An extension apparatus was at once applied, a bag of sand fourteen pounds weight forming the medium of extension; the injured limb was carefully compared with the sound one, and traction made on it until both limbs were of equal length. An extensive destruction of the soft parts took place, the popliteal space and posterior part of the upper third of the leg became boggy, and in about a fortnight an extensive sloughing of the integuments and muscular tissues occurred leaving a rough jagged gaping wound, about nine inches long and four wide, exposing the tendons of the ham-string muscles, and denuding a portion of the upper part of the tibia of the muscles that were attached to it. From this wound exuberant granulations sprouted, and had to be reduced by free cauterizing with sulphate of copper. At one time an accumulation of maggots took place, whose incubation was favoured by the hot sultry weather of the season, but they were rapidly destroyed by carbolic lotion.

The apparatus, however, was so adapted to the leg that the free



application of poultices, caustics, carbolic applications, and such remedies as from time to time were necessary could be applied with the greatest facility, and without pain or inconvenience to the patient. The crushed bone rapidly consolidated, but the injury to the soft parts prolonged recovery. The patient was allowed to get up on crutches at the termination of the twelfth week, and was discharged from the hospital on 29th November, without the slightest shortening of the limb.

CASE II.—J. C., aged sixty years, a coal miner, working in the Australian Agricultural Company's colliery, on 4th December, 1872, while at work in the pit, had his right femur fractured obliquely below the lower third by a fall of coal from the roof. It being ten o'clock on a dark rainy night when I saw the case, I put up the injured limb temporarily in a M'Intyre's splint. Next day I applied the extension apparatus. As he had no hair mattress in the house, I put him on a palliasse, with a pillow under his buttocks, and a flat pad under the tendo-Achillis, to protect his heel from pressure. When I measured the injured limb in M'Intyre's splint there was shortening to the extent of two inches, but a few minutes after the application of the extension apparatus, steady traction having been made, the shortening was remedied, so as not to be detected. During the seven weeks that this patient was confined to bed I had not the slightest trouble, the apparatus not requiring any other adjustment, except that during the third week I substituted one brick for the two that I used as weights for extension.

At the termination of the seventh week he was able to get off his bed, and on the 31st March I gave him a certificate, permitting him to resume his work at the colliery.

*Observations.*—These two cases give a good idea of the results that may be attained by a method of treating fractures of the femur much to be recommended for its simplicity and facility of application. It fulfils satisfactorily the three indications—(1.) Coaptation and fixature of the fragments; (2.) Moderate extension; and (3.) Gentle compression and support of the limb; besides which, for the treatment of compound, comminuted, or complicated fractures of that bone, it presents unusual advantages for the manipulation and examination of the parts, as well as for the application of dressings during treatment. In the use of Dr. Gurdon-Buck's apparatus a perineal band is required to afford means of

counter-extension, and the straps of plaster used for extension of the limb are applied without the intervention of a foot-board to the sole of the foot. The perineal band is not only inconvenient, but irritates the perineum, and imparts to the patient a shackled feeling of constraint most undesirable, as it harasses the sufferer, already condemned to a tedious confinement, which it is the surgeon's interest to make as bearable as possible. Raising the foot of the bed about five inches, so as to allow the gravitation of the body towards the head to act as a means of counter-extension, fully answers the required purpose. The addition of a foot-board of the shape shown in the sketch (Fig. 1) serves three purposes—being wider than the sole of the foot it keeps the straps of plaster used for extension from unduly pressing on the ankles; when it is properly bandaged on to the foot and ankle an additional “grip” is obtained of the limb for extension; finally, the long flat edge, which is to be supported by the mattress, prevents rotation.

Each individual case of fracture of the femur may require a slight modification of treatment in the minor details; for instance, different measures may be adopted to keep the heel of the injured limb off the mattress or palliasse, so that no injurious effects should result from pressure. Again, where the patient is supplied with a badly-made mattress, it may be desirable to let the inferior part of the foot-board slide on two parallel slips of wood, after the manner that a carrier would slide a package along a skid. Such minor modifications as these may be made by the surgeon, in accordance with the requirements of each case.

Hamilton<sup>a</sup> states that, “in case of an oblique fracture of the femur occurring to an adult, whose muscles are not paralyzed, but which offer the ordinary resistance to extension and counter-extension, and where the ends of the bones have once been completely displaced, no means have yet been devised by which an overlapping and consequent shortening of the end of the broken bone can be prevented.” He also affirms that “the average shortening in simple fractures, where the best appliances and the utmost skill have been employed, is about three-fourths of an inch.”

Mr. Carston Hothouse, in his admirable article in “Holmes' System of Surgery,”<sup>b</sup> published only three years ago, gives, as the

<sup>a</sup> Hamilton on Fractures and Dislocations, 3rd ed., p. 405.

<sup>b</sup> Holmes' System of Surgery, 2nd ed., vol. ii., p. 865.

result of his investigations in the London Hospitals, according to the present treatment adopted there, that in fractures of the femur shortening of the limb follows in ninety per centum of adults admitted for treatment, and admits that even this per-centage is probably underrated. In the practice of this experienced surgeon at the Westminster Hospital, he has not succeeded in treating a single fracture of the adult without shortening, nor has he found in upwards of one hundred specimens of the fractured bones that he has examined in the various museums of the English metropolis *more than one* in which there was not some shortening. So convinced is Mr. Holthouse of the impossibility of union without shortening, that he has committed himself in his article to the following dogmatic assertion, in a foot-note alluding to reported cases in which writers aver that cases have been cured without shortening:—

“Surgeons are still to be found hardy enough, or ignorant enough, to repeat the fallacies which have been so often refuted, and to vouch their success in the cure of oblique fractures in the adult without shortening.”

Such strong language from a London surgeon of eminence may be justified by the results obtained by the use of the long splint, of which he has given an elaborate description in his paper. That he has had no personal experience of Dr. Gurdon-Buck's apparatus is, however, self-evident, from the fact that he dismisses that gentleman's admirable appliance by copying the originator's drawing of the apparatus, and giving it a passing notice of barely thirteen lines.

I take it that the remarkable statement of Mr. Hamilton, and the very strong language used by Mr. Holthouse, justify the publication of these successfully treated cases of fracture of the femur, in which union has taken place without any perceptible shortening of the limbs; and if the method that is now so fully explained be more generally adopted, I have no doubt but Mr. Holthouse will have an opportunity of modifying his language in his future writings on the subject.

I wish to add that the apparatus now described by me was introduced into use at the Newcastle Hospital by Mr. Richard Harris. In sketching the apparatus I made use of Dr. Gurdon-Buck's original drawing, merely modifying it by omitting the perineal bandage, raising the foot of the bed on bricks, adding the foot-board, and re-arranging a few other details.

ART. II.—*Report on Small-pox in 1871, '72, and '73, at the Workhouse Hospital, Mullingar; with some Remarks on Treatment.* By DILLON KELLY, M.R.C.S.E., Medical Officer to the Hospital.

THE appearance of small-pox was heralded here by the *empusa muscæ*. Scarcely a window but exhibited one or more specimens of “the magic circle in white” surrounding these exuviae.

In Church-lane, South, more especially, the windows were rich in such remains, and I possess some slides cut from them, in which they appear as granular gummatous-looking *debris*, and some of which still contain casts filled with a reddish fluid like blood.

The first case of variola occurred here in June, 1871. The patient, aged seventeen, was housemaid in a tavern frequented by cattle-dealers from every part of Ireland, and the house is a model of cleanliness, but unfortunately is situate on the lowest level of one of the drainage basins of the town, and within a few feet of its common sewer. The case was apparently sporadic, unless contagium was imported by some of the cattle-dealers, two of whom were from an infected district in Dublin, and remained some days. Here it may not be out of place to observe that the first case of cholera, in 1866, was that of the former mistress of that house, who died of it.

The second and third cases of variola were imported—the first by a gambling tramp, who came to the races; the second by a blind beggar, whose infant died of it in hospital here. He was decidedly poisoned by alcohol, his mother having dosed him from the period of his attack until his death, as she was taken into hospital with him, and concealed it when coming in.

His urine was transparent, a deep amber with reddish tint and purple periphery, and developed a vinous odour, remaining free from decomposition for two months, when fungi made their appearance for the first time—thus, to a great extent, proving the correctness of the alcoholic hypothesis.

The fourth case was due to sleeping on the straw on which the family of the gambling tramp had lain, and every succeeding case from that period to the early spring of the present year was evidently the result of contagion purely, and traceable from house to house through the media of wakes, intercourse, and card-playing.



There was one house more particularly a rendezvous for young card-players, and very dirty, from which two cases came into hospital in a dying state.

Yet it was not until the spring of the present year that variola visited Mullingar in its purely epidemic form—more than a dozen cases of a severe type occurring simultaneously in different quarters of the town, marking conclusively the date of its occurrence.

The first cases were purely variola, the eruption pustular, and some complicated with purpura. These were succeeded, as the epidemic progressed, by a varioloid complication, in which both pustules and vesicles were represented, or the eruption was of a composite character, and consisted of pustulo-vesicles, the pustules beneath, the vesicles above.

Then, as the epidemic advanced, its virulence became less, and gradually the pustules of variola disappeared—their places being occupied by the vesicles of varioloid, and these, in their turn, yielded to those of varicella, which wound up the epidemic here, and heralded its departure.

The urines of three of the patients, who succumbed within a few days of each other, whilst the epidemic influence was at its height, were so similar in their general characters and reactions that I trust a short description of their characters will not be deemed out of place.

Jane C., aged twenty-five. 18th day, urine albuminous, thick and creamy in appearance, pale and tawny in colour, odour fishy, periphery streaked and banded; the streak inferior, dirty white, and beaded, and underlying a pale band with reddish tint; re-action feebly acid, gravity 1030; assuming a pinkish tint, and developing the odour of putrescence when reacted on by sulphuric acid.

The urines of M'Loghlin, aged seventeen, and of Hayden, aged twenty-three, were similar in their fishy odour, their streaked and banded peripheries, and their putrescent odour under sulphuric acid reaction; but the colour of Hayden's urine was a pale amber, its deposit mucoid, and its periphery a pale orange.

All these urines were loaded with triple phosphate, and that of Hayden contained some uric acid in addition; in fact, in every case of variola in which I examined the urine I found more or less of triple phosphate, and the side of the urinal in Jane C.'s case was coated with a translucent, starchy, violet-tinted muffing.

In reference to the term periphery, it implies the outer circle of the surface of the fluid where it comes in contact with the glass

cylinder in which it is contained. In consequence of the interrupted pressure of the atmosphere the fluid ascends in that position on the surface of the glass urinal in the form of a triangle or prism.

By holding the glass urinal at right angles with the light, and just above the level of the eye, the natural prism thus formed will give a rough spectrum analysis of the colours, which give their tints to the urine in the ratio of the oxidation of the substances which produce them, and so will convey a pretty accurate idea of the nitrogenous tissue waste just then taking place in the system.

The colour of the urine, too, is also significant—thus, a light amber, either transparent or opalescent, with deep orange periphery, a light pinkish deposit, an acid reaction, a gravity of about 1024, with odour of weak broth, or odourless—the urine meanwhile assuming a light purple tint, and a sweet brine-like odour, when reacted on by sulphuric acid—is generally favourable.

The urines of Jane C., M'Loghlin, and Hayden are typical examples of the unfavourable specimens most frequently met with.

The purely epidemic appearance of variola here was accompanied by currents of air apparently deficient in oxygen, and similar in odour to that exhaled from the urines of Jane C., M'Loghlin, and Hayden.

Thus I perceived a similar odour on two occasions the same evening, quite close to a large open window looking into the street, in a large room thoroughly ventilated and scrupulously clean, as if passing from the window towards the open door. Three servants had been almost simultaneously sent to hospital from that house but a few days before. The odour was exactly similar to that exhaled from the banks of rivers in the early morning or in the evening, after a shower; but, as Dr. Aitkin described an odour, which I consider to be similar, under the term "fishy," I have adopted his nomenclature.

I had but two cases of varioloid—one pure, in which the malpighian layer of the skin solely was implicated, and in which the whole surface of the face, the upper portion of the chest, the backs of the forearms and hands, were densely covered with vesicles, which, on desiccation, left deposits of albuminoid secretion so indurated that the skin felt as rough as a nutmeg grater over these portions of it; yet there was not the slightest pitting on convalescence. The second case was complicated with a copious effusion of the deepest purple, in which the bulbs of the fingers and

toes, the whole of the great toes and a bunion, the matrices of all the nails, and the whole of the soles of both feet, were of a blue-black colour, from ink-like tint of the underlining serum; yet there were no petechiæ.

The vesicles over the whole surface were also filled with a similar coloured secretion, which gave it an appearance, when desiccation was complete, as if it had been sprinkled over with black varnish, which had been allowed to dry upon it.

Both these cases terminated most favourably, but their convalescence was protracted and tedious from previous debility.

The composite cases, or those of variola complicating varioloid, were most frequent just before the appearance of variola, or during the latter days of the epidemic, and heralded its departure.

In these the eruption was pustulo-vesicular; and in one case, in which the superimposed bullous or confluent vessels were transparent, the semi-transparent pustules, arranged like pseudo-loculi, were visible beneath them.

In such cases the malpighian secretion may be contained in dome-shaped vessels alone, when the consecutive purulent secretion will appear in small pustules beneath, or when from any cause the pustules beneath give way, then the purulent and albuminoid secretions combined will be both contained in one and the same pseudo-vesicle. Then the various tints of these purulent and albumino-purulent secretions possess their own peculiar significance.

Thus the golden yellow is the result of the presence of oil globules, and the index of a good constitution; whilst the dirty, tawny, semi-transparent, or white wax-like secretions of variola are all indices of danger.

The pasty and white wax-like, however, are the most dangerous, as they show a leucæmic state of system, a blood so poisoned and feeble as to be incapable of acting as a carrier of oxygen, and a constitution totally incapable of reaction.

In four cases of variola, complicating purpura, and menorrhagia, both inguinal regions, from the anterior superior spinous processes of the ilia to the bend of the trochanters, were throughout packed with petechiæ, and so densely in the case of Jane C., that they almost seemed black.

In one of these cases, a girl of twelve, the menses appeared for the first time, were very profuse, and continued almost up to the last moment of her existence.

In the year 1871 there was but one case of small-pox admitted

into hospital, there were seven in 1872, and thirty-seven in 1873—forty-five cases in all.

There was one death in 1871, two in 1872, and ten in 1873—thirteen deaths in all, four of whom were infants.

Two came into hospital, one dying on the second, the other on the fifth day; the remaining two came in with their mothers, and caught the disease in the house.

A male, aged seventeen (the son of an intemperate father), died on the seventh day; a female, aged seventeen, immediately after her confinement, died on the fifth day; another female, aged twelve, already alluded to, died on the fifth day; another, aged twenty, who was sent to her friends, a distance of eight miles, when she became ill, and sent back by them to hospital the day after, died on the seventh day—most likely from the shock of her expulsion from home and the fatigue of the double journey.

Two fatal cases—namely, Fox, a male, aged twenty-seven, and Simpson, a female, aged fifteen—pulled through the disease, Fox dying of pyæmia, productive of multiple abscesses, on the sixty-second day, one of which, seated in the bronchial glands, burst into the trachea; and Simpson, of phthisis, on the one hundred and fourth day—these, therefore, are not included in the deaths from variola.

*Treatment.*—The treatment in mild cases consisted of simple saline mixture or acetate of ammonia, nitrous ether, and camphor mixture, with beef-tea and diluents freely given, but guided by the previous habits of the patient—if alcoholic stimulation was habitual, alcohol was then administered.

In some severe cases the time-honoured remedy, dilute sulphuric acid in infusion of roses, was resorted to, and it proved not only grateful, but at the same time most effectual in removing both thirst and head-ache in several cases. Thus in the case of Grey, who had intense head-ache, intolerance of light, and thirst when she came in, a single dose at once checked the morbid action producing these symptoms, whether zymotic or specific, and they never returned.

It also acted decidedly like a specific in some purpuric and hæmorrhagic cases.

In variolo-mania, which was very prevalent, from three to four grains of tartarated antimony, with from thirty to forty minims of acetum opii, according to the strength and violence of the patient, quieted the excitement in every case; and although delusions and



hallucinations still continued in some cases, yet the active symptoms invariably yielded to a single dose.

There was one case more particularly in which the calmative power of the antimony was crucially tested. It was that of a married woman, aged thirty-two, and nursing, who came into hospital in the vesicular stage, and in a state of the greatest excitement. She kept roaming all night through the ward, had "runaway" action of her heart, with a pulse up to 180, and imagined that one of her acquaintances, then three months dead (to which she repeatedly alluded), was sitting just outside the door and watching her. As these symptoms were so serious, I had the advice of my son, who had considerable experience of such cases, and he fully concurred in the necessity for calming the excitement by the exhibition of antimony and opium—to be followed by camphor and hyoscyamus on succeeding nights if necessary; and half-drachm doses, three times a day, of tincture of digitalis, to restore the heart and bring down the pulse. Two grains of the antimony and forty minims of acetum opii gave her a quiet night; although she slept but little, her dead acquaintance had disappeared, and her pulse had fallen to 120. Under the camphor and hyoscyamus treatment at bedtime, and the digitalis during the day, her pulse steadily decreased in frequency, and her hallucinations disappeared. Having ascertained that she was accustomed to stimulants they were administered, and she made a most perfect recovery.

Here it may not be out of place to remark, that, with his full concurrence, I gave three grains of tartarated antimony and forty minims of acetum opii to a typhoid patient, a man aged thirty-five, with tympanitis, cæcal tenderness, and gurgling, but without diarrhœa, who kept roaming through the ward from the time of admission until the following evening, a period of about twenty-three hours. In this case it acted like magic, as shortly after taking it the patient became calm, went to bed, and made no further attempts at leaving it until convalescence was established. The cæcal tenderness and tympanitis also rapidly disappeared, but whether *post hoc* it is difficult to say; his convalescence, however, was tedious, but his recovery was perfect.

I was induced to give the antimony in these cases from the calmative power I have seen it exert on most maniacal patients when in states of noisy excitement; also on the authority of the late lamented Dr. Graves, whose opinion on that subject, already conclusively proved, is further strengthened by these cases.

In variola, complicated by purpura, and menorrhagia, I found the sulpho-chloride of iron most effectual.

The patient was an inmate of the house, squat, stout, and an epileptic, covered all over with petechiæ small as points of pins, and apparently beneath the malpighian layer, yet her recovery was complete, and her convalescence rapid in the extreme.

The sulpho-chloride mixture consisted of one drachm each of sulphate of iron and hydrochloric acid and twenty ounces of water, half a wineglassful every second, third, or fourth hour. It was most agreeable, and would be taken in mouthfuls if permitted.

As to stimulants, I found them positively injurious, two cases having succumbed under their use. I believe them to be applicable only when, unfortunately, the constitution has been accustomed to their abuse; in cities, however, they are unavoidable.

In reference to this subject it is a most extraordinary coincidence, if nothing more, that of the four malignant cases with purpuric condition of the inguinal regions, three were servants in public-houses, all of whom died; the fourth was the epileptic, who recovered. In fact, every adult servant from a public-house who came into hospital labouring under variola succumbed to the disease.

---

ART. III.—*Practical Observations.* By ROBERT FITZMAURICE, L.K.Q.C.P.I., L.R.C.S.I., &c.; Physician to the County of Kerry Fever Hospital.

I. REMARKS ON SMALL-POX.

II. NOTES OF A CASE OF EMPYEMA, IN WHICH SEVERAL ASPIRATIONS WERE PERFORMED—FATAL ISSUE.

*Remarks on Small-pox.*

AFTER the exhaustive discussions at the College of Physicians last winter, on the small-pox epidemic that recently appeared in Dublin, it would be superfluous on my part to enter into a detailed account of an epidemic that prevailed in this town and neighbourhood from November to May last, and which presented characters identical with those so ably described by the medical men attached to the Dublin and Bray hospitals, and which were made available for such cases. My remarks will be, therefore, brief, and confined to a few practical observations, having reference principally to treatment.

For the six months ending 30th April, 1873, there were 117 cases of small-pox admitted under my care to the County Fever Hospital. Of this number 44 were vaccinated—73 unvaccinated. Of the 73 unvaccinated 34 died, being a little under 50 per cent. Of the 44 vaccinated 3 died, giving a mortality of about 7 per cent. Of the number admitted there was not one re-vaccinated case. From my experience of the epidemic I would be disposed to divide it into four typical cases, each having a certain range of degree. The first and worst case was that in which there was a purplish, measly rash, that never filled, with an hæmorrhagic tendency, delirium, and rapid pulse, indicating *early* a very poisoned state of blood. This case was very fatal, and the treatment most suitable was sedatives, nourishing diet, stimulants in moderate quantity, and chlorate of potash in large doses. Second case presented at first a purplish rash, which after a time (about the sixth day of the eruption) became pasty, and filled with a grey-coloured fluid, confluent all over the body, but more especially on the face, arms, and legs. This was a most serious case, and very fatal, and though some very bad examples recovered, the greater number died—some from laryngitis, some rapidly poisoned, and others, after a hard struggle (an adult lived 19 days), ultimately succumbed to the poisoned state of blood. This case was almost invariably seen among the unvaccinated. The treatment was—nourishing broths *early*, when the rash first appeared, a moderate amount of stimulants, chlorate of potash in large doses, and opium in large doses night and morning, and dredging with flour, as recommended by Dr. Darby, of Bray. The third I would call an inflammatory case, rash ending in numerous large pustules, with red areola all over the body, well filled with white purulent matter, not confluent, but quite close together, with high fever and great soreness of skin. This case was almost certain to recover, and the treatment consisted of salines, low diet, and opium night and morning, till the pustules burst, or began to wither up, when nourishing diet and a small amount of stimulants were allowed. Fourth case, the discrete form of small-pox, that recovered without medical treatment, was seen in some unvaccinated cases, but principally among the vaccinated. The antiseptic treatment by the sulpho-carbolates and sulphurous acid, so assiduously carried out by Dr. Foot at the Meath Hospital, I cannot report favourably of. I certainly only tried it in a few cases, but seeing no result, and believing that precious time was lost, I soon changed to chlorate of potash—a

remedy I have more faith in, and which I can with more confidence recommend. I gave it in large doses, ʒiii. to the pint of water in 24 hours, and I have seen some very bad cases of the second class recover while taking it; at the same time as symptoms appeared they were met with other remedies, especially opium, which I consider as indispensable in this disease. It was given in all stages, but was more applicable to the secondary fever; and if much delirium was present, application of a few leeches beforehand to the temples was attended with the best results. A child ten years old got at first 10 or 12 minims of laudanum night and morning, and an adult 30, which was increased to 40 and 60 minims night and morning, as the case may be, till restlessness and irritation subsided. I am not in favour of much stimulants in small-pox. I don't think they are at all expended on this disease, as seems to take place in typhus fever. One adult left the hospital nearly blind from sloughing of the cornea, the result of intense ophthalmia. If the inflammation had been detected early, the loss of sight might have been prevented by the application of a strong solution of nitrate of silver, but as in all bad cases the eyes are closed from swelling of the lids, the inflammation was not recognized till sloughing set in. In all such cases the globe should be examined daily. All the outer coats of the cornea sloughed away, leaving the inner coat, which in one eye burst from slight pressure, depositing the crystalline lens on the cheek. The second eye, as the healing process advanced, became more and more opaque, and on leaving hospital the patient could distinguish between light and darkness, and barely notice the outline of objects. I believe during the late epidemic in Dublin there were some lamentable cases of this kind. One child who was unvaccinated, and whose father died at home of confluent small-pox, recovered with the complication of pneumonia, for which a blister was applied with the best effects. Mustard cataplasms and linseed poultices were useful in laryngitis; leeches seemed to do harm.

*Notes of a Case of Empyema, in which several Aspirations were performed—fatal issue.*

In May last I was called upon to visit J. C., aged twenty-two, the son of a respectable farmer, and come of a healthy family, who had been suffering from cough and oppression for six weeks previously. Present state—pale, tall, and thin; tongue a little furred toward the back; cough, and frothy expectoration; some shortness



of breath when he walks, none when quiescent; pulse from 104 to 108 and rather full; no sweats; appetite bad; some belching. Physical examination—tympanitic dulness over the antero-superior portion of the left lung; respiration here almost absent and tubular toward the apex; no crepitus, but rhonchus sometimes; dulness on percussion over all the posterior part of the left, and at the side from the axilla downwards tympanitic dulness, evidently, from the stomach. The respiration over the posterior part of the lung very weak, and at the upper part tubular; absence of vocal vibration posteriorly; puerile respiration all over the right lung; dislocation of heart to the right side. Treatment—nourishing diet, mercury given to touch the gums slightly, and the affected side painted with tincture of iodine, commencing behind, and when this part became sore, painting in front. Symptoms and signs very much the same to the 12th June, and on this day found as follows:—Tongue clean; pulse about 120; appetite somewhat better; expectoration a little more yellow and heavier, though still frothy; other symptoms the same. The left side of the chest looks rounder and fuller than the right, especially from the clavicle to the nipple, but on measurement there is no appreciable difference. The physical signs continue almost the same, except that on the second rib, and from that to the clavicle the respiration is more of a deep-seated, cavernous character, and the same is heard in the left supra-spinous space. Ordered nourishing broths, cod-liver oil, and Griffith's mixture. No important change in symptoms or signs to the 31st July, when succussion is heard. The respiratory murmur is louder posteriorly, but there is complete absence of respiration anteriorly, where percussion gives the same sound as before. Symptoms the same. There is pain and tenderness at the left side, below the fold of the pectoral muscle. Aspirated at the back, between the seventh and eighth ribs, and six pints and a half of a creamy, foetid, purulent matter taken away. Tonic treatment continued. August 11th—Lost cough and expectoration after aspiration, and was able to lie on either side, the decubitus being on the back before; other symptoms the same as on the 31st July. Antero-superior portion of chest morbidly clear, and no respiration heard over this part; metallic tinkling and respiratory murmur heard posteriorly. Aspirated again, and two pints of same fluid taken away, and a pint of a solution of carbolic acid,  $1\frac{1}{2}$  per cent., injected, and drawn off soon again. August 16th—Pulse 112 to 116, weak; feet and ankles

swollen bowels moved three times a day, but not fluid, no oppression; very little cough and expectoration; emaciation extreme; antero-superior part of left chest looks sunken; percussion sound over this part clear, and some respiratory murmur heard over cartilage of third rib; respiratory murmur, with coarse crepitus heard posteriorly, but no metallic tinkling, which latter sound was heard at the side from the axilla downwards. Heart beats more under the sternum; crepitus heard at the end of expiration on or about the place where the heart was resting at the right side. Aspirated, and one pint and a half of solution of carbolic acid injected and drawn off again. After aspiration respiratory murmur heard posteriorly without crepitus; continue nutritious diet and tonic treatment, which included extract of malt. August 22nd—Symptoms and signs about the same as on the 16th. Aspirated, and about one pint and a half of fluid taken away, and an attempt made to pass an uterine sound into the chest to cut down upon, for the purpose of inserting a drainage-tube, but it was found difficult to get it between the ribs, and a curved silver catheter which was introduced could not be felt at the base of the lung, probably from the thickness of the false membrane—any further attempt to insert drainage-tube was therefore abandoned. September 3rd—Pulse 108 to 116, weak; he looks more emaciated and sunken; cough and expectoration increased as chest began to fill again; other symptoms the same. Physical signs of more fluid, and two pints of fluid taken off, and ten ounces of a saturated solution of boracic acid in decoction of cloves (*amykos-aseptin*), as recommended in a late number of the *Irish Hospital Gazette* (August, 1873), injected, and drawn off in ten minutes; percussion sound natural over the left chest anteriorly, and very little, if any, respiration heard here. Vesicular murmur loud posteriorly, metallic tinkling from the axilla downwards as before; tonic, and nourishing diet continued. September 7th—Very emaciated; no swelling of feet to signify; tongue clean; bowels too free; pulse 112 and weak; can lie on either side; some more cough, and expectoration of frothy, muco-purulent matter. Physical examination—no dulness anywhere except at the postero-inferior part of lung, loud vesicular murmur anteriorly, and at the humeral angle metallic tinkling, and same sound heard at a distance on the dorsum scapulæ, and at the side from the axilla downwards; respiratory murmur posteriorly. He said he had the taste of the decoction of cloves in his mouth; aspirated with a small needle, and half a pint of fluid taken away, and nine ounces of decoction of cloves and boracic acid

diluted with water injected, and about one-third of it drawn off—the remainder of it could not be found, though a larger needle was used; continue treatment as before, varying tonics. He sank in a fortnight afterwards. No *post-mortem* examination was allowed.

*Remarks.*—As this was a chronic case, and the patient lived at a distance from town, the visits, as may be seen, were “few and far between,” and therefore the accuracy consequent on a daily record of his state was wanting; but, as I believe from what has been noted a few practical lessons may be derived, I am tempted to give what I call “fragments” of the case, as taken from my notebook. It was evident on the first examination that there was a vast pleural effusion—the antero-superior tympanitic dulness, which gave of itself the idea of a large excavation, being confirmatory of this view, but there were two other questions in the diagnosis of serious import, upon which very much depended the prognosis and treatment—1st. What was the nature of the fluid? 2nd. Was there lung complication? The physical signs gave no help to discover the state of lung, as the pressure of so large an amount of fluid effectually prevented its expansion, and the symptoms—namely, white tongue, bronchial expectoration, full pulse, 104 to 108, absence of hectic and dyspnœa, freedom of the opposite lung—together with the family history, were opposed to the view of tubercular deposit. Mercury was, therefore, administered to touch the gums slightly, and when the object for which it was given—namely, to excite the absorbents—was not attained, and when the expectoration became muco-purulent, it was immediately stopped, and tonic treatment substituted. The friends were told that as it was impossible to ascertain the state of the lung, no decided opinion as to the ultimate result could be given; that as the case advanced absorption might take place, and then physical signs might be made available for this purpose; but that, in any case, the existence of a large quantity of fluid in the chest with quick pulse, must be necessarily looked upon as serious. After some months of treatment, succussion was heard on the 31st of July, showing that some absorption had taken place, whereby place was given for air, either secreted, or from chemical change, or from fistula in the lung, and the respiratory murmur being much louder posteriorly, was a further proof of expansion, but the quick pulse found at each visit, with the other symptoms continuing about the same, silenced any signs of amendment which were so eagerly sought for by the poor father, who was hopeful to the end, and

sceptical as to the nature of the case (supposing it to be consumption), till the evacuation of the fluid by aspiration proved to him that the diagnosis was correct. In this case the aspirator worked admirably, at one time the canula and trocar was tried, but the fluid came so slowly that the aspirator had to be added on. It was evident that under its use the lung was gradually expanding, as the respiratory murmur was becoming louder, and was also, towards the termination of the case, audible anteriorly, proving that the flat tympanitic dulness heard here resulted from effusion, as diagnosed, and not from an excavation in the lung. The rapid filling of the chest after aspiration indicated a large secreting surface, and was against the supposition of pneumo-thorax with fistula. Was the taste of the decoction of cloves in the mouth, and the frothy expectoration in favour of it?

If such a case occurred in my practice again, where there was so large an effusion, I would be inclined to aspirate at once, for two reasons—1st. To relieve the lung early; and 2ndly, to ascertain the nature of the fluid, as a guide to treatment. If an effusion was met with extending only to the inferior angle of the scapula or a little higher, I do not think I would be justified in aspirating till all other remedies to cause absorption failed. I think, in the event of a large effusion, it is a question whether we should not be satisfied, for a time, with the evacuation, occasionally, of only a portion, say half of the fluid, to allow of gradual expansion of the lung, at the same time combining medical treatment and the local application of tincture of iodine; a great deal depending, of course, on the symptoms present. Another question also arises:—What about inserting a drainage tube immediately, or would it be well to inject the chest with some antiseptic fluid, either with or without a drainage tube? The course of injecting was pursued, latterly, in this case, as the secretion of fluid went on so rapidly, to which, by abstracting from the blood, may be attributed, probably, the fatal result. The opportunity of daily injection, as practised in some cases lately recorded in the *Irish Hospital Gazette*, was not afforded in this case for the reasons above specified.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

*A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL D. GROSS, M.D., LL.D., D.C.L., Oxon., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c., &c. Illustrated by upwards of fourteen hundred engravings. Fifth Edition, Greatly Enlarged and Thoroughly Revised. In Two Volumes. Philadelphia: Henry C. Lea. 1872.

WE congratulate the author of this great work upon the high literary position it has attained, as well as upon the fact, that it has not yet been surpassed for leading to a correct appreciation of the principles of surgery, and as a work of reference for the actively-employed practitioner.

It is a pleasure to scan its pages for other than didactic reasons, almost every page containing evidence of the absence of that narrow-minded selfishness so conspicuous in the works of some of our English contemporaries. We regret that we have been unable to extend this notice to the degree that we should have wished. We have been compelled, therefore, to confine ourselves to some passing comments in our course through the work.

The first chapter contains many excellently described illustrations of irritation, sympathy, and idiosyncrasy. Congestion is then discussed. The subject, as Mr. Gross observes, "is one of no ordinary difficulty;" in fact, "there are no two points in pathology concerning which there still prevails a greater amount of confusion; for what one author considers as congestion, another with equal confidence calls inflammation, and conversely; leaving thus the young and inexperienced practitioner in painful doubt not only in respect to the nature of the disease, but, what is far worse, in regard to its proper mode of management."

There is much difference of opinion among writers on inflammation as to the point of departure of the inflammatory process. We

ourselves believe that some molecular change, be its cause what it may, antecedes the congestion. Mr. Gross, on the other hand, considers that "in all acute inflammations, whatever their cause or situation, active congestion is a necessary antecedent of the morbid action—one of the first links in the chain of the malady." In what inflammation essentially consists, it is barely necessary to say, is one of the most difficult problems in pathology. So that, after all, we are obliged to limit ourselves, Mr. Gross remarks, to "an examination of its causes, symptoms, and effects, or, in other words, to a rigid analysis of its appreciable phenomena and consequences." Wandering or migratory leucocytes (white blood cells) have not been overlooked in the chapter on inflammation. We may here mention that their erratic vagaries were long ago suspected by Addison, although to Cohnheim "is due the credit of having correctly interpreted his own observations, and of developing the researches of his predecessors," and thus proving that the white cells wander from the vessels into the irritated and inflamed tissues. This chapter is worthy of Mr. Gross's pen. Before we leave it we should state, that he does not believe that the non-depletive treatment of inflammation is to be attributed to any modification of type of disease, and to a gradual diminution of the strength of the American people. He believes that inflammation possesses the same characteristics now as it did formerly, and it is equally clear to his mind that patients bear depletory remedies quite as well now as they did then. It would be difficult, indeed, to reconcile the present mental and bodily activity of Mr. Gross's countrymen, with deteriorated bodily fluids and structures.

The observations on the various forms of abscess, including "multiple abscess, pyæmia, or purulent infection," contain the most recent views on pyæmia, including those of Virchow, who, we think with Mr. Gross, "has thrown more light upon the entire subject than any other pathologist." But we must agree with Mr. Gross, that "of the intimate nature of the poison of pyæmia nothing whatever is known."

In the treatment of mortification Mr. Gross favours the use of blisters, of which he speaks as follows:—"The approach of gangrene may often be averted, certainly materially checked, by the timely use of a blister, large enough to cover in not only the whole of the inflamed part, but also a portion of the healthy skin, and retained sufficiently long to effect thorough vesication." He is "satisfied from observation that there is generally no more

efficient remedy." There is much uncertainty, however, in deciding on a matter of this kind, from the doubt that must be uppermost in the mind as to the exact condition of the part to which the blister is applied. We imagine that in a case in which mortification is imminent, warmed turpentine would be preferable to a blister as a local application. The blister, it is but right to mention, "was a favourite means in this affection in the hands of Physic."

Mr. Gross is an advocate for amputation in gangrene when the death of the part has been completely arrested, provided the condition of the patient's system does not contra-indicate it. In traumatic gangrene, he follows the practice attributed to Larrey, does not wait for the line of demarcation, and operates at once, provided, in this case also, there is no contra-indication to the operation.

Mr. Gross is likewise an advocate of amputation in senile gangrene when there is "a well-marked line of demarcation; and, indeed, not even then, unless it is perfectly evident that there is sufficient strength of the system to bear the shock of the operation." He has "seen several cases where amputation was succeeded by the most happy results before nature had made any attempt to cast off the slough, and that, too, under circumstances apparently not at all promising as it respected the powers of the constitution." We need scarcely say, that to enable us to form an accurate judgment on cases like these, the diagnosis should be unexceptionably accurate. We have not been so fortunate as Mr. Gross, in meeting with a case of undoubted senile gangrene that would justify amputation. Indeed, in the last case of this description which came under our observation, although the gangrene was exceedingly limited, amputation, owing to the debility of the patient, would have been a most reckless undertaking. Mr. Gross himself admits, that "when the operation is performed prematurely, or before the system has sufficiently recovered from the exhausted condition consequent upon the gangrenous action, the disease will generally re-appear within a few days after upon the stump, or death will follow from sheer prostration." We need scarcely remind the reader that, in the endeavour to form an opinion upon a question of this kind, accuracy of diagnosis, here also, is a crucial point.

We join Mr. Gross in his condemnation of the absurd divisions made of ulcers by many modern writers. He substitutes for these divisions the more rational and more easily remembered division into

“common and specific.” “It would puzzle,” he remarks, “the most profound pathological anatomist to discriminate between some of these classes of ulcers, as, for instance, between the irritable and inflamed, the sloughing and sloughing-phagedenic.”

With regard to skin-grafting and other plans based upon it, in the treatment of chronic ulcers, Mr. Gross alludes to the experiments of Guyon, and of others who have shown “that skin-grafting is nearly as successful when practised with epidermic scales as when it is done with bits of skin.” Muscle-grafting, he further tells us, is equally as successful as skin-grafting in the treatment of ulcers:—“It has generally been supposed that the success of skin-grafting is essentially due to the presence of epithelium; but that this is not true has been clearly proved by the recent experiments of Professor Howard, of New York, who has obtained precisely similar results from grafts of bits of muscle, inserted into granulating wounds.”

The chapter on Tumours is a very excellent *résumé* of, probably, all that is authentic upon the subject of these growths. Some of the illustrations are very good, and the treatment recommended judicious.

Mr. Gross has modified his opinion and practice in the question of operation for cancer. “I was formerly,” he states, “greatly opposed, in carcinomatous affections, to operative interference merely as a palliative measure, believing that the pain and risk consequent upon its employment would not counterbalance the beneficial effects. After more mature experience, however, I came to a different conclusion, and for years past I have, in numerous instances, used the knife where there was not the slightest prospect of a permanent cure. The class of cases to which this procedure is more especially adapted are those in which there are excessive pain, loss of appetite and sleep, and great discharge of foul, offensive matter, contaminating the air of the patient’s apartment, and rapidly undermining the vital powers.”

Mr. Gross’s views coincide with our own regarding the connexion between phthisis and scrofula. “I have long been convinced,” he says, “of the identity of these diseases, and of the fact that the only actual difference between them depends, not upon any difference in the morbid action, but solely upon the difference of structure, tubercular disease sharing the same fate, in this respect, as ordinary inflammation, and as the other heterologous deposits.”

Mr. Gross’s observation of phthisis, in America, leads him to



say that children in that country suffer comparatively seldom from phthisis, by which, we suppose, he means pulmonary consumption.

We have too often, we are sorry to say, seen in Ireland true tubercular pulmonary phthisis in very young people, in whom it is liable to be mistaken, owing to modifications in the auscultatory phenomena of the phthisis of early life. In the phthisis of young children we lose the assistance to be derived from the presence of hæmoptysis in the early stage of expectoration and of profuse sweating. Moreover, in these subjects the disease is frequently masked by symptoms arising from tuberculization in other localities; indeed, it is sometimes so latent that the deaths caused by it are attributed to marasmus by the unsophisticated.

The views of a surgeon so distinguished as Mr. Gross on the effect of the syphilitic poison, in developing struma, cannot fail to be of much interest, in consequence of the great prominence lately given to this question. He mentions, that a "close and careful study of scrofula during a third of a century, in public and private practice, has forced upon" his "mind the strongest conviction that many of the so-called cases of the disease, as they are brought under our observation, are simply examples of a syphilitic taint of the system in its more remote forms."

He refers to a matter connected with vaccino-syphilis, which ought to impress upon the mind of the vaccinator how great should be the precaution taken in the selection of vaccine lymph. "During the late war," Mr. Gross records that "numerous cases" occurred in which syphilis was communicated by vaccinating soldiers with lymph from infected persons.

Mr. Gross is evidently in favour of the "unicity theory" in connexion with the syphilitic poison. We have always believed that but one poison is engaged in the propagation of the syphilitic disease, and, therefore, we had much pleasure in reading Mr. Gross's observations on this subject. In the domain of surgery there is no affection, the description of the natural history of which, appears to be so warped by prejudice as syphilis. Opinions are formed upon it, and logic is thrown aside. In no point is this more striking than as regards the so-called infecting and non-infecting sores. We have repeatedly seen confirmatory evidence of the following experience of Mr. Gross:—

"My observations lead me to infer, that while there are really two varieties of chancre, the indurated and the soft, as described by modern

syphilographers, they do not, by any means, possess the properties which they ascribe to them. The hard chancre is unquestionably most frequently followed by general symptoms, but to maintain that it is so exclusively, is what I cannot, without further light, admit. So far from giving any adhesion to such a doctrine, I have had the most unequivocal evidence, in numerous instances, of the infecting property of the soft chancre. Indeed, I am satisfied that some of the very worst cases of secondary and tertiary syphilis that I have ever witnessed were cases of this description; originating generally in very small sores upon the prepuce or head of the penis, perfectly soft, very superficial, manifesting no disposition to spread, and soon completely disappearing."

Those who have seen much of syphilis will recognize this description to be an unprejudiced history of the consequences that may result from the hard and the soft sores. Again, as he also observes, both the hard and the soft sore not unfrequently follow inoculation from the matter of mucous tubercles.

Mr. Gross speaks cautiously of the syphilization mode of treating syphilis, and indirectly warns the surgeon of the legal risk that may be incurred by pursuing this treatment, such as in a case that recently occurred in France. We have ourselves frequently practised syphilization, and without having to regret it. In all our cases marked improvement ensued upon it.

The chapter on wounds contains some useful illustrations of the different forms of sutures, including Rigal's method of substituting india-rubber or gum elastic rings, instead of silk, for confining the pin in the twisted suture. The india-rubber, however, is objectionable, because the pressure made by it cannot be so well regulated as that made by the silk or the thread.

The treatment of gun-shot wounds inculcated by Mr. Gross will probably meet with the approbation of the majority of surgeons.

In Mr. Gross's observations upon wounds inflicted by poisonous insects and by venomous serpents, there is no allusion to the treatment of persons bitten by the latter, by injecting solution of ammonia into the veins, as practised by Halford, apparently with great success, in Australia, although so unsuccessful in India. It would be interesting to have some information on the practice in this work, provided it has been largely tried in America.

In addition to the cases given by Mr. Gross in proof of the communicability of glanders to man, and from man to the so-called lower animals, we may mention the following instances that came under our observation:—Many years ago a policeman was

walking through a lane, and seeing a stable-bucket full of sparkling water, he, being thirsty, knelt down and drank some of the water. Symptoms of glanders appeared in a few days afterwards. The source of his infection having been sought for, it was ascertained that the bucket from which the man drank, belonged to a stable infected with the glander poison.

In corroboration of Mr. Gross's statement, that the "mere inhalation of the infected air of the patient's apartment may give rise to the malady," we shall also mention another case we observed, and which at the time could be attributed only to this mode of infection.

The subject of the case was an old woman, who inhabited a badly floored room over a glandered stable. She died glandered. A donkey was inoculated in one nostril with some of the discharge taken from her nose. It also died glandered.

Mr. Gross does not allude to a colourless or blanched areola that surrounds the glander pustule, and is considered by some surgeons pathognomonic of its nature. We have seen it occasionally only, and, therefore, cannot believe it to be a very constant appearance.

Railway accidents have become so common, no systematic surgical work could be considered complete without containing some allusion to the injuries resulting from them. Mr. Gross's observations on these injuries are by no means the least valuable part of this work.

There can be no doubt that distressing remote effects sometimes follow railway accidents, nevertheless, as they frequently lead to appeals to juries for damages, too great care cannot be taken by medical witnesses engaged in cases of this kind, to be most searching in the examination of those professing injured persons who may complain of subjective symptoms only. There are too good grounds for the belief, that malingerers have, before now, been practised for the extortion of money from railway companies, by persons who have been in trains when accidents have occurred to them.

It would be well were the following remarks, on the qualities a surgeon should possess, a little more frequently inculcated by surgical teachers, to try and quench that thirst for operations unfortunately now too general and too reckless:—

"If he," Mr. Gross writes, "is not honest in his purposes, or scrupulously determined, in every case, to act only with an eye single to the

benefit of his patient, and the glory of his profession, he is not worthy of the name which he bears, or fit for the discharge of the solemn duties which he assumes. In a word, such an operator is not to be trusted, for he will be certain, whenever opportunity offers, to employ the knife rather for the temporary *eclat* which may follow its use, than for the good of the individual whom he unnecessarily tortures." . . . . "The question, in every case, should be, is an operation necessary to save life, or to place the individual in a condition calculated to promote and insure his recovery."

We cannot go so far as Mr. Gross does, in his condemnation of operations for malignant diseases, believing that in many of these cases operation gives a respite from mental anxiety, and relieves the patient, for a time at least, of a loathsome and painful malady. We have already mentioned, however, that he has modified his opinions upon this matter.

With regard to the troublesome bleeding that sometimes comes from veins after amputations of the thigh and leg, Mr. Gross gives some interesting information. "Great prejudice," he writes, "exists against ligating veins, but experience has shown that the fears that are entertained upon this subject are, if not entirely ill-founded, greatly exaggerated. The largest veins, as the internal jugular and femoral, have repeatedly been tied with impunity. I have myself never hesitated to adopt this treatment when occasion seemed to demand it; and in no instance, as far as I remember, have any evil consequences ensued. The testimony of other surgeons is equally conclusive upon the subject." This is in accordance with the teaching of many excellent recent writers on surgery.

"That the fears of exciting dangerous inflammation of the veins by the application of a ligature are utterly groundless, has been conclusively demonstrated by Dr. S. W. Gross, in an elaborate communication in the *American Journal of the Medical Sciences* for January and April, 1867. Of forty cases of ligation of the internal jugular vein analyzed by him, death was fairly ascribable to the ligature in only four, all due to secondary hæmorrhage, coming on about the time of the separation of the thread. In not a single instance was diffused phlebitis excited. Since the publication of his paper Dr. Gross has analyzed twenty cases of ligation of the external jugular vein, and fifteen of the axillary vein, which add additional testimony to the safety and efficiency of the ligature, inasmuch as all did well."



These cases are of much importance, a knowledge of which would be of use in strengthening the hands of the surgeon in dealing with troublesome venous hæmorrhage at the time of, and after, certain operations.

There is one remedy recommended by Mr. Gross for the treatment of pyæmia after amputation, which we do not consider likely to be suitable in the treatment of pyæmia as seen in Ireland. "Mercury," he says, "in the form of calomel, is sometimes useful, and may be administered in doses of three grains every four, six, or eight hours, with a view to rapid, but gentle ptyalism." This drug, thus administered, may agree with pyæmics in the New World, but we hesitate to recommend it for the blood-poisoned inhabitants of the mother country.

Mr. Gross, from an analysis of the statistics at his disposal regarding the relative safety of chloroform and ether as anæsthetics, gives the mortality from chloroform inhalation at 1 in 2,723 cases, and from ether inhalation at 1 in 23,204 cases. Curious to say, that notwithstanding this low mortality from the ether inhalation, he expresses his regret that chloroform should be so little employed in America, "because a fatal instance, produced, for the most part, by maladministration, is occasionally reported, as if death in surgical operations never arose from any other cause."

We believe that opinions are still very much divided, even in America, on the relative safety of these anæsthetics. We can hardly think, however, that the deaths from maladministration of the chloroform vapour could have been so numerous as to account for the enormous difference in the mortality caused by these anæsthetics, as shown by Mr. Gross's figures.

We have just alluded to Mr. Gross's recommendation of the use of mercury in pyæmia. As we proceed in the work we find, that he also uses this drug in erysipelas, so as to produce the "slightest possible ptyalism;" but he carefully avoids it in "old, worn-out subjects, and in persons exhausted by intemperance and dissipation." As these probably are the kind of erysipelatous subjects most frequently admitted into our Irish hospitals, mercury, so far as its specific effects are concerned, should, with them, at least, be carefully avoided. We have more than once seen old people sink rapidly after the accidental production of ptyalism by mercurial ointment used as a local application in this disease.

The pathology of boil, as given by Mr. Gross, agrees with the results of our own investigations, and is totally at variance with the

untenable views of Gendrin on the core of boil, and of Nélaton on the core of its allied disease, anthrax. Mr. Gross favours the subcutaneous division of the indurated tissues in anthrax. When the skin, however, is much disintegrated, then he has recourse to incisions. Sometimes he has seen the cure hastened by systematically applied compression.

Mr. Gross's application for burns seems to us to be worthy of extended trial, from its adhesiveness, which should render it a good application for excluding air from the burned parts. It is common white paint, of the consistence of cream, made with white lead and linseed oil. He has used it in numerous instances and has never witnessed any toxæmic effects from the lead. This used to be a favourite application of the late Sir Philip Crampton to ulcerated cancer.

It would be well for the young surgeon to keep in mind Mr. Gross's suggestion in his observations on sebaceous tumours, that no operation for their removal should be performed without "due preparation of the system." He has seen several cases of erysipelas follow upon the operation, which, if not prevented, may be greatly modified in its severity by a previous preparation of the patient. We are acquainted with a case in which fatal tetanus ensued upon the removal of a very small sebaceous cyst from the scalp.

We might add to the list of tumours occurring in striated muscles, given by Mr. Gross, the very rare myoma, formed of transversely striated muscular fibre, of which Virchow has a few specimens.

Mr. Gross seems to have little confidence in tobacco and its active principle nicotine in the treatment of tetanus, there being no mention of them in this work; and yet, in the cases of tetanus tabulated by Curling, there were more recoveries of persons treated with tobacco than with any other remedy. We fear, however, that we must agree with Mr. Gross that "after all that has been done by modern science for the cure of traumatic tetanus, the humiliating fact stares us in the face that, of the many remedies that have been paraded before the profession, there is not one that is worthy of special confidence. An analysis of the cases tabulated by Dr. Cowling, clearly shows, as Professor Yandell very justly remarks, that the results of treatment by different remedies are so nearly alike that no one agent can properly be considered as possessing any decided superiority over any other. All, in fact, are equally of apocryphal virtue."

We have recently stumbled upon a case of traumatic tetanus in Dr. Charles A. Gordon's work on "China from a Medical Point of View," in which recovery followed upon opium-smoking treatment.

Acupressure, it appears, has made but little progress in America, few, if any, prominent surgeons employing it there. Mr. Gross, himself, seldom tries it, being satisfied "with the harmless character of the ligature." Although we rarely practise acupressure in a serious operation, we have occasionally found it useful as a hemostatic in traumatic hemorrhage. We have, for example, occluded, at once and permanently, both the occipital and temporal arteries divided by injury, the bleeding from which had previously baffled other hands. The hemostatics to which we are partial in operations are torsion and the *presse-artère*.

Mr. Gross favours the subsulphate of iron, as a styptic, in preference to the perchloride of iron. It has the great advantage over the perchloride of not being irritating, and is not so liable to cause suppuration.

According to Mr. Gross, atheromatous degeneration of arteries is very rare among native Americans, but is common among the Irish and English immigrants. A similar immunity from amyloid degeneration of the blood-vessels has likewise been observed in the native Americans.

The immigrant Irish and English being so prone to atheromatous degeneration, it is remarkable that aneurism is most infrequent in the southern states of North America, which are peopled by a mixed population, "made up from all parts of the world."

We, Irish, have the unenviable privilege, Mr. Gross tells us, of suffering "more frequently from aneurism than any other race." He does every justice to the labours of Dr. O'Bryen Bellingham (Dublin), who, with his able associates, "created a new era in the treatment of aneurism, hardly less brilliant than that of the ligature."

Mr. Gross thinks that the method of suddenly arresting the flow of blood into the aneurismal sac is preferable to the method of only diminishing the calibre of the stream through it. In fact, he advocates the production of a sudden coagulation of the blood in the sac, in preference to the deposit of laminated fibrine. He bases his opinion upon some rapidly successful cases recorded by Mr. William Murray, Dr. Mapother, and Mr. Heath. The plan of

suddenly and completely closing the artery leading to the sac, in preference to its incomplete closure, is practised by many Irish surgeons. In England, also, the advocates for rapid coagulation of the blood in the aneurism seem to be increasing in number. It should not be forgotten that suppuration of the sac is one of the risks attendant upon this mode of dealing with the tumour.

The observations on aneurism of the subclavian and innominate arteries are of peculiar interest, remarkable for the jealous care Mr. Gross has taken, not to omit notice of those who have joined in the surgery of aneurism engaging these vessels. We find here the instructive case which occurred in the practice of Dr. J. Kearney Rodgers, in which the left subclavian artery was ligated for aneurism. This case is important from the fact that "although the patient finally perished from hæmorrhage, yet the ligated vessel was completely closed by an adherent coagulum."

Mr. Gross concludes his description of aneurism with an account of the operation for ligation of the peroneal artery.

Mr. Gross depicts the anatomical characters of acute phlebitis thus:—"Swelling, opacity, and pulpiness of the internal membrane of the veins, with uniform redness, varying from light pink to deep florid. The middle and outer coats soon become profoundly injected, and their proper substance, although at first preternaturally soft and humid, is at length rendered so dense and firm that the vein feels like a hard contracted cord. The cavity of the inflamed vessel is filled with clotted blood, sometimes blended with pus or lymph, and in many cases it is lined by a false membrane, susceptible, under certain circumstances, of organization. Instances occur in which the pus is infiltrated into the substance of the vein, or collected into small abscesses beneath the serous lining. The pus of phlebitis is generally an imperfectly elaborated fluid, containing a large quantity of plastic matter, and comparatively few characteristic globules."

We give this paragraph in full, the views advanced in it being different from those of other recent writers on phlebitis. There are pathologists, for example, and with these we are disposed to agree, who hold that the lining membrane of a vein is incapable of originating the inflammatory process, although they admit that this may commence in the outer walls of the vein. Writers who are of this opinion, attribute the discoloration of the innermost membrane, observed in certain stages of phlebitis, to blood-staining. They would also consider, what Mr. Gross calls "pus of phlebitis,"



altered and softened thrombi rather than true pus. Be this as it may, we must admit, that there are practical surgeons who would consider Mr. Gross's description of venous inflammation a correct interpretation of phlebitis.

For the treatment of varices of the lower extremities, he recommends the making of issues, with Vienna paste, in the course of the affected vessel. He considers this to be the safest mode of dealing with them; but, at the same time, cautions his readers against "rushing headlong into measures which, if injudiciously used, may lead to the worst results."

Should the surgeon be urged to meddle with varices, we would suggest, in preference to the use of the Vienna paste, a careful injection of the vein with the perchloride or the subsulphate of iron. More people have probably escaped death from this treatment, than from any of the other numerous operative measures invented for the treatment of the varicose condition of veins.

The Vienna paste, in other hands than those of Mr. Gross, has produced sores, troublesome and very difficult to heal. It is probably nearly as dangerous as the ligature or the division of the vessels.

Mr. Gross's experience of the frequency of transverse fractures of the shafts of long bones, differs remarkably from that of some writers on fractures. He has found these transverse fractures "extremely rare." Moreover, he tells us that, "in the extensive osseous collection of Dr. Mütter, now in the College of Physicians, there is not a solitary specimen of the kind, and his own is equally barren." "There is reason," he further says, "to believe that many of the so-called cases of transverse fractures of shafts of the long bones are in reality oblique fractures, approaching more or less closely to the horizontal line, and yet not strictly falling within it." If the reader will take the trouble to look at the illustrations of transverse fractures in some special works on fracture, he may be induced to agree with Mr. Gross. Mathematically speaking, a perfectly transverse fracture of the shaft of a long bone is not often seen.

Mr. Gross advocates spine trephining when the spine is fractured, "if the symptoms were such as to render it certain that the lesion was accompanied by depression."

When we remember, however, that the spinal column requires great violence for its fracture—that the contained cord may be thereby irreparably damaged, independent of depressed bone—and

that fracture may be seated in the column anterior to the cord which no operation could with safety reach—we are constrained to dissent from Mr. Gross's recommendation.

"Of 38 cases," he tells us, "tabulated by Dr. John Ashhurst, in which this procedure has been employed, including those of Barton, Rogers, Potter, Blackman, Hutchinson, and Stephen Smith, 29 died, 3 were relieved, 2 were not benefited, and the result is unknown in 4. In the cases reported as relieved, the improvement was only partial, a circumstance that might have been expected when it is recollected how seriously the spinal cord is generally injured by the depressed fragments."

Mr. Gross mentions a remarkable case, which, in a medico-legal point of view, is of much importance. In this case the sexual power was preserved after serious injury of the spinal column. "The man, who lived fifty-three years in a perfectly paraplegic condition, consequent upon a fracture of the twelfth dorsal vertebra, was married at the age of thirty-six, and became the father of six children. The spinal cord was completely atrophied below the seat of the injury. The capacity of coition was unimpaired, but there was never any venereal orgasm. Sensation existed only in front of the abdomen, of the scrotum, and of the thighs. The urine was expelled, after the first four weeks, by a painful effort of the will, aided by a loop of rope tightly twisted with a stick over the hypogastric region."

Mr. Gross, like the editor of S. Cooper's "*Surgical Dictionary*," as we mentioned in our recent notice of that work, attributes to Nélaton the pistol splint for the treatment of fracture of the lower end of the radius. He has been led into this error, by English writers probably, many of whom have made a similar mistake. Nélaton does not mention, in either edition of his work on "*Pathological Surgery*," the pistol splint, which, there are reasons for supposing, is the invention of Guérin, in whose journal it was first described.

Mr. Gross records an instructive example of intra-uterine fracture of the tibia; and as a further illustration of intra-uterine pathology, we may refer to a specimen of intra-uterine periostitis, with separation of the upper tibial epiphysis, recorded in the number of this Journal for November, 1859.

Mr. Gross forms one of the very limited number of recent writers who correctly delineates Dupuytren's apparatus for the treatment of Pott's fracture, as it is called.

In intra-capsular fracture of the neck of the thigh-bone, according to Mr. Gross, "*crepitation*, the most valuable sign of all fractures generally, is rarely wanting." "Indeed, it can only be absent in the impacted form of the lesion, or in those cases where the ends of the fragments remain still partially in apposition with each other. It is usually discoverable immediately after the accident, and may, of course, be produced as long as the fracture continues ununited. In order to determine its existence, it is necessary to bring the ends of the broken bone fully in contact by extension and counter-extension, when, upon rotating the limb, it will at once declare itself." These words are almost identical with those of Sir Astley Cooper on this symptom. But we have not been so fortunate as to elicit crepitus in this injury with the facility mentioned by the author. The manœuvre recommended by Sir Astley Cooper and Mr. Gross is founded upon the supposition that the upper fragment of the bone remains in the exact position it occupied at the moment of the fracture. This we believe to be so rare, that to rub the fractured surfaces together when the limb has been extended and rotated, must be a matter of but doubtful success. Malgaigne, we must admit, mentions that the fragments never completely abandon one another, and he at the same time says, that he has never, during life, succeeded in producing crepitus in this fracture.

Some of Mr. Gross's countrymen having, we may safely say, published more cases of what they call osseous union of intra-capsular fracture of the neck of the femur, than the surgeons of any other country, it will, we are sure, interest the reader to have the opinion of our distinguished author on these American specimens of supposed osseous-united intra-capsular fracture:—

"It would, perhaps, be going too far to deny altogether the possibility of bony union in intra-capsular fracture of the thigh-bone, and yet I have certainly never seen what I could regard as an unequivocal example of the kind. I have examined most of the specimens in this country purporting to be cases of osseous consolidation, and in no instance have the appearances been such as, in my opinion, to justify such a conclusion."

Even in Europe, we may add, some of the published specimens of osseous union have been called in question by Malgaigne, Mr. Adams's specimen (Dublin) not excepted, the appearances of which he attributes to rachitic alterations of the cervix rather than to

veritable fracture. Malgaigne, however, had not the opportunities for examining Mr. Adams's specimen that Mr. Gross has had for examining the American ones.

Very recently, the late Mr. R. W. Smith published, in the Transactions of the Pathological Society of Dublin, the illustration of a specimen which has all the appearances one would expect to find in an osseous-united intra-capsular fracture of the neck of the thigh-bone. A description of this specimen will be found in the number of this Journal for January, 1873, p. 99.

Although we are indebted to Goyrand for perfecting the subcutaneous method for the removal of foreign bodies from the knee-joint, Mr. Gross has unintentionally overlooked the claims of M. Chassaigne, who, as Mr. Adams (London) has shown, preceded Goyrand in the development of this method.

Amputation at the hip, in extreme cases of coxalgia, does not meet with much favour from Mr. Gross. He writes on this point:—

“I should, nevertheless, hesitate before undertaking so grave a procedure, the more especially as the same end may generally be more readily attained by the more simple and less dangerous operation of excision.”

If the case be suitable for excision, owing to the limited amount or length of the femur diseased, amputation would be a most unjustifiable step. It should be recollected that this operation has only a place in surgery, when the femur is diseased so far down that excision could not afford any chance of a useful result.

In the article on Chronic Rheumatic Arthritis, Mr. Gross does ample justice to the labours of Messrs. Robert Adams and R. W. Smith, of Dublin. But we cannot think that the treatment recommended by Mr. Gross for the early stage of chronic rheumatic arthritis could be very suitable for the class of subject usually attacked by it in the old countries. In America, he tells us, “there are few cases of chronic rheumatic arthritis which will not, in their earlier stages, bear active depletion by the lancet, purgatives, diaphoretics, and antimonials.”

Mr. Gross very accurately states, in his description of chronic rheumatic arthritis of the shoulder, that “the humerus is generally a little raised, as well as somewhat nearer the middle line than natural, the scapula is unusually mobile, and a remarkable depression exists at the posterior part of the joint.” We miss, however,



allusion to the mistake that has been made by many English writers, who have attributed these deformities to a partial luxation of the humerus upwards, from accident. We have entered fully into this matter in our notice of Cooper's Surgical Dictionary, already referred to.

Luxation of the clavicle upwards, which is so extremely rare that many of the best surgeons, as Mr. Gross states, "formerly doubted the possibility of its occurrence," has been recently observed in Dublin, a specimen demonstrative of the accident having been exhibited by the late Mr. R. W. Smith at a meeting of the Pathological Society of Dublin. A dozen examples of this luxation have not yet been recorded. The description of Mr. Smith's case will be found in the number of this Journal for December, 1872, p. 450.

Mr. Gross does not speak favourably of Jarvis's surgical adjuster for the reduction of dislocations. "I have never," he says, "employed it in recent dislocations, and in the repeated trials which" he has "made with it in those of somewhat long standing it has not been" his "fortune to meet with any marked success."

We have heard of the unsuccessful use of a modification of this adjuster in five or six dislocations. It being an instrument of extraordinary power, it should, as Mr. Gross inculcates, "be used with care and discretion."

In the description of Malgaigne's subcoracoid dislocation of the humerus, Mr. Gross does not place the head of the bone in the position in which Malgaigne has himself placed it. In this surgeon's subcoracoid dislocation, the tip of the coracoid process should touch the head of the bone, so as to divide it into two equal parts. Were the head of the bone in the position mentioned by Mr. Gross, *i.e.*, "below and to the inner side of the coracoid process," the dislocation would then be Malgaigne's intra-coracoid dislocation, although in it the head of the bone does not altogether clear the coracoid process.

The chapter on anomalous dislocations of the hip, although a short one, contains some most instructive examples of such cases.

Provided there was no error in the diagnosis of the nature of the accident the cases of reduced old dislocations mentioned by Mr. Gross are remarkably interesting. Thus, in a case of sciatic dislocation, he tells us, that "Dr. Sayre effected the reduction of a sciatic luxation by manipulation at the end of nine months." How wonderfully fortunate was the subject of this accident, whose

acetabulum remained for nine months in a sufficiently capacious condition to be re-occupied by the head of the bone! We think, nevertheless, with Mr. Gross, that fortunate cases, such as this was, "must be regarded merely as so many exceptions to a general rule, and nothing more."

We are not satisfied that the rule laid down by Mr. Gross to trephine the skull in every case of its punctured wound, "without the slightest regard whatever to the character of the head symptoms," will meet with universal approbation.

After alluding to Dr. Stromeyer's practice in the Schleswig-Holstein war—a practice founded on the dogma that trephining, "independently of the mischief upon the tissues during its performance, the admission of air to the contused portion of the brain and its membranes, greatly augments the danger of inflammation," and to the corroborative testimony furnished by the unfavourable results of trephining in the English army in the Crimea—Mr. Gross gives some evidence, afforded by the researches of Dr. S. W. Gross, in support of the view that "the great mortality after trephining is due to the extent of the primary injury and its effects upon the brain and its membranes, and not to the operation itself." This, he considers, has been clearly and satisfactorily elucidated, by Dr. S. W. Gross in a paper published in the *American Journal of the Medical Sciences* for April, 1867. "Thus, of 160 cases of trephining in army practice, 97, or 60·62 per cent., were fatal; while of 573 serious gunshot injuries of the skull treated by expectancy, 426, or 74·34 per cent., perished. Of 126 cases in which fragments of bone or various foreign substances were removed with the elevator, forceps, or, less frequently, with Hey's saw, 56 recovered, and 70, or 55·55 per cent., died. A comparison of these results shows a ratio of recoveries, after trephining alone, of 39·38 per cent.; after the elevator, forceps, and saw, of 44·45 per cent.; after all operative measures combined, of 41·61 per cent.; and after conservative treatment, of only 25·66 per cent. Since the cures after surgical interference and expectancy are in favour of the former by 15·95 per cent., it follows that the mortality from gun-shot injuries of the skull must be referred to cerebral disorders, and not to the operations practised for their relief. Of 252 cases of trephining, derived from private and hospital practice, European and American, analysed by Dr. Gross, 133, or 52·77 per cent., perished, the mortality being less than that of army practice by 7·85 per cent."

Notwithstanding the apparent force of these statistics in favour

of trephining, we cannot believe that the relative mortality of trephining and non-trephining in the injuries of the head, alluded to by Stromeyer, can be accounted for by referring the mortality in the trephined cases to cerebral disorders, independent of the operations practised for their relief. Stromeyer, who was Surgical-chief of the Schleswig-Holstein army during the campaigns of 1848 and 1851, recommended that the operation of trephining should be discontinued, even in compound fracture of the skull with depressed bone. He attributes the fatal results after the application of the trephine to the influence of the air in the production of suppurative inflammation. During the earlier years of his surgical experience, although he did not meet with a case in which trephining had been successfully practised, he still adhered to the views inculcated by Sir A. Cooper, Brodie, and others, that, in complicated fractures of the skull, trephining must be resorted to, because of the threatened formation of pus. Further experience, Mr. William Adams tells us, led him to doubt the correctness of these views, principally because it became apparent to him that the air must exercise a deleterious influence upon a contused part of the brain, no matter whether it be admitted by trephining or by any operation for removing loose fragments of the skull. After the battle of Kolding, in Schleswig (April 23, 1849), there were eight gun-shot fractures of the skull, with depression, and more or less considerable brain symptoms, in the hospitals of Kolding, Christiansfelde, and Hadersleben. In all these cases, with only one exception, the detachment of the fragments was left to nature. The eight patients recovered perfectly under antiphlogistic treatment.

In the year 1850, after the storming of Friedrichstadt, two young surgeons came under Stromeyer's care with gun-shot wounds of the head, accompanied by deep depression; they recovered perfectly under non-operative treatment.

From the two campaigns of 1849 and 1850, Dr. Stromeyer possesses the notes of forty-one gun-shot fractures of the skull, with depression, in which there was no doubt about the existence of fracture of the skull, because it was denuded. It is, however, doubtful whether the brain or dura-mater was injured; because this can only be ascertained by the escape of cerebral matter from the wound, or by extracting fragments at an early period. Of these forty-one cases, only seven terminated fatally. Thirty-four were cured, of whom one only had been trephined by Dr. Ross, in

consequence of inflammation coming on several days after the wound. Stromeyer recommends the cautious extraction of perfectly loose fragments and foreign bodies. The removal of impacted balls, he says, should not be attempted; nor is the external wound to be at first dilated under any pretext whatever. The exclusion of air, the presence of which would favour the decomposition of the secretions of the wound, is indicated in all cases. He relies upon cold applications to the head, and antiphlogistic treatment, especially venesection.

Now let us compare the results of the trephine operations during the Crimean war with those which followed non-trephining in the Schleswig-Holstein army. These results of the Crimean practice are not likely, as Mr. Adams states, to alter the opinions of Stromeyer. "My friend and colleague," Mr. Adams writes, "Mr. Blenkins, of the Guards, informs me that the cases of recovery from trephine during the first year of the war were three; the number of cases that were operated on is unknown. In the second year there were twenty-eight cases trephined, with four recoveries."

We have seen many cases of depressed fractures of the skull do well without operative interference; and Mr. Adams informs us that "evidence of frequent recovery, in cases of depressed fracture of the skull, may be obtained by examining the skulls in the various museums of London. A considerable number of such skulls exist in the museum of St. Thomas's Hospital (London), and made an anti-trephine impression upon" his "mind when he was engaged in describing them" . . . "for the never-to-be-published catalogue of that collection."

Surgery not being an exact science, fallacies are peculiarly liable to insinuate themselves into surgical statistics, which we have always received with reserve. We hesitate, therefore, to place so much reliance upon them in forming an opinion on this question, as Mr. Gross appears to have done.

That the surgeons of Guy's Hospital (London) do not now use the trephine so freely as Sir Astley Cooper used it, is evident from the following observations of Bryant in his recently published "Surgery:"—

"It would thus appear that in simple or compound *uncomminuted* depressed fracture from a local injury, no operative interference is called for, unless associated with marked symptoms of compression of the brain, or extravasation of the blood between the bone and



the dura mater. In compound *comminuted* fracture, with or without symptoms of brain compression, it is wise to remove all fragments, and well to elevate the bone, when it can be done without adding to the local mischief. In all other cases, as in fracture of the base, no surgical interference can be justified."

"Did space permit, many cases might be quoted to illustrate these points. Cases of fracture of the skull, depressed bone, and recovery, are numerous; indeed, it is most remarkable how much depression of the bone may exist without giving rise to brain symptoms, and I am almost tempted to believe that depressed bone by itself never gives rise to marked symptoms of compression, and that when these are present hæmorrhage exists with it."

Although we are not prepared to go so far as La Franchi of Milan, who, three hundred years ago, said that, in fractures of the skull, everything depends upon "the assistance of the Holy Ghost, which the surgeon should implore above all things," we think, nevertheless, that the trephine has been too often used indiscriminately and recklessly. Indeed, Mr. Gross himself admits that the operation of trephining is not free from danger, "as is proved by the fact, that it is often fatal when it is performed for the relief of epilepsy and other severe nervous symptoms, although a distinction," he conceives, "should be drawn between such cases and those involving recent injuries."

As regards its results for the relief of epilepsy, he states that he has "had occasion to perform the operation four times, with the effect of one cure and three deaths," and he "witnessed its execution in three other cases, all of which terminated fatally."

To those who have not leisure to study elaborate monographs on meningitis and myelitis, Mr. Gross's observations on these affections will be most useful, referentially speaking at least.

Our experience differs from that of Mr. Gross on the frequency of suppuration in spinal meningitis and myelitis. He has found that "pus is seldom present, even in the more protracted cases" of spinal meningitis and myelitis. This may be the case in myelitis, but in cerebro-spinal arachnitis we have on many occasions seen pus effused on the inflamed membranes.

There can scarcely be a second opinion as to the accuracy of Mr. Gross regarding the doubtful diagnostic value of pain in the back in strumous disease of the spinal column:—

"Pain in the back," he writes, "is seldom an early symptom, nor in any stage of the complaint a very reliable one; for, in the first place, it

does not by any means always clearly indicate the existence of organic disease, either of the vertebræ, of the spinal cord, or of its membranes; and, secondly, it is well known that these structures may be seriously involved, and yet not occasion any pain whatever. Nevertheless, pain in the back, especially if circumscribed and persistent, should always be regarded with suspicion, and provoke the most diligent inquiry into the nature of the disorder. The cases are not at all uncommon in which the very first appreciable evidence of tuberculosis is the projection of the spinous process of one of the vertebræ, without any apparent antecedent disturbance of the general health, or the utterance of any decided complaint by the child."

Pain and tenderness, as the author conveys to us, are not necessary concomitants of strumous disease of the spinal column. We have repeatedly seen the disease proceed to well-marked angular curvature without being accompanied, or even preceded, by either pain or tenderness.

We have not so much confidence in the use of issues in this disease as Mr. Gross seems to have. Our experience of them has led us to the conclusion that they are only, if at all, warranted when there are symptoms of irritation of the spinal cord. When these do not exist, or when they have ceased, having previously existed, issues should be dispensed with. Indeed, Mr. Gross says, that the issue is "more particularly adapted to the earlier stages of the affection, and to children of good constitutional stamina. When the system is worn out by long suffering it does harm instead of good." We cannot bring to our mind ever having been so fortunate as to see either caries or tuberculosis of the spinal column in persons of good constitutional stamina.

If we consider that nature, when successful, maintains the continuity of the spinal column by throwing out stalactitic osseous growths from the vertebræ immediately above and below the disintegrated ones, and that the greater the gap to be bridged by these growths, the greater the demand upon her, we are not partial to any of the spinal extending apparatuses recommended by some surgeons in the treatment of this strumous disease of the column. Should, however, the surgeon wish to try appliances of this kind, the one delineated by Mr. Gross seems admirably suited for the purpose.

The chapter on psoas abscess contains an accurate description of this variety of abscess, and very practical rules for its management.

In Mr. Gross's account of tumours of the back, he mentions a large fatty tumour which was removed from the back of a negress by Dorsey, and which weighed twenty-five pounds. Although the fatty tumour, as he states, is generally immediately beneath the skin, remarkable exceptions as to its position are now and then observed. We have removed a lobulated fatty tumour from underneath the rhomboid muscles and a portion of the scapula. Among other exceptional positions, the fatty tumour has also been found in the tongue.

The operations for the relief of neuralgia of the superior maxillary nerve described by Mr. Gross, and with which he associates the names of the American surgeons, Carnochan, Blackman, and W. H. Mussey, are of much interest. We think, however, that as some of the operations are very elaborate, it would have made this work more useful if the author, instead of referring his readers to the *American Journal of the Medical Sciences* for the results of Carnochan's "severe and complicated" operation, had incorporated them with the text.

When we inform the reader that, for Carnochan's operation, the surgeon will require "chisels and bone nippers, two trephines, one three-quarters of an inch in diameter, and the other half an inch, the latter being intended for perforating the posterior wall of the antrum, the results should be very promising, indeed, to warrant an operation so severe as that for which these instruments are required, namely, division of the superior maxillary nerve beyond Meckel's ganglion, and particularly when it is remembered that a sub-cutaneous division of the nerve, as practised by Langenbeck, Hahn, and Hueter may be followed by the relief of the neuralgia.

Mr. Gross applies the term epithelioma to corroding lupus of the face, which is not quite in accordance with the teaching of the majority of surgical pathologists. Indeed, according to our observation, the differences in histological characters between this corroding lupus and epithelioma, or epithelial cancer, are very remarkable, no part of the tissues involved in and bounding the lupoid ulceration having appeared to us to contain any epithelial cancerous structures.

Mr. Gross, in speaking of the use of belladonna in ophthalmic surgery, mentions that he had heard "of two infants, labouring under congenital cataract, being destroyed by wearing a belladonna plaster upon the forehead and temple." It is to be hoped that these cases may be of use in impressing upon the student the advisability of caution in even the local application of this drug.

Mr. Gross's suggestions on the treatment to be followed when a foreign body, lodged in the interior of the eye, cannot be extracted, seem to us to be most judicious:—

“Where useful vision yet remains, and the foreign body is creating no irritation, the case should be kept under close observation, and the patient cautioned to seek aid upon the appearance of symptoms of irritation in the sound or uninjured eye. Where useful vision is destroyed, and the foreign body yet remains, giving rise to destructive inflammation, the eyeball should be extirpated to remove the cause of a probable sympathetic ophthalmia in the sound eye.”

Equally judicious are Mr. Gross's observations on local applications in conjunctivitis:—

“Locally, none but the mildest remedies should be employed. It is a great mistake, yet one which is constantly committed, to use strong applications to the eye in every form and stage of the inflammation. Often” has he “seen a simple conjunctivitis, which in a few days might have disappeared spontaneously, converted into a violent, obstinate, and protracted disease by the untimely use of an improper collyrium! If a collyrium be admissible at all, it is only, as a general rule, after the morbid action has been, in some degree, subjugated by other means, when it has assumed a sub-acute character, or become chronic. When the symptoms are urgent and threatening,” he “sometimes departs from this rule, but even then seldom without regret. In the purulent and gonorrhœal varieties of the affection, many ophthalmic surgeons urge the employment of strong collyria at an early stage of the attack, on the ground of their beneficial influence in controlling inflammation.” He has used them “himself in such cases, but seldom without a conviction of their injurious effects.”

We infer from Mr. Gross's columns of comparison between rheumatic iritis and syphilitic iritis, that he considers the latter form of iritis to be necessarily always associated with “papular eruptions, sore throat, and other evidences of syphilis.” Our experience in such case would be opposed to that of the author on this point. Syphilitic iritis, according to our observation, is not necessarily accompanied by eruption. We have had many cases in which the iritis was almost a solitary syphilitic symptom, careful examinations of the patients having been required for accurate diagnosis of the nature of the ophthalmia.



As an instance of the frequency of the cysticercus in the eyeball in Prussia, the following evidence is given by Mr. Gross:—

During his visit to Berlin in 1868, "Professor Von Graefe performed his one hundred and twenty-second operation for the removal of such a body from the eye." Its great frequency in Prussia, the reader is probably aware, has been "ascribed to the consumption of raw or uncooked pork by the people of that country."

Professor Donders, of Utrecht, who was present with Mr. Gross at the operation, stated that he had never seen an example of the cysticercus (in the eye) in Holland.

Mr. Gross, in his account of foreign bodies in the nose, mentions that Dr. Hays, of Philadelphia, has reported a case in which a glass button was retained "upwards of twenty years" in the naris, "keeping up constant irritation." This shows the necessity for a careful examination of the nose in children when there is persistent discharge from it, particularly when the discharge comes from one naris only. A friend of ours had a case a few years ago in which a cherry stone was lodged in the naris of a child for four or five years, when it was at last discovered by him, after it had been overlooked by those who had the previous management of the patient.

We can confirm the observations of Mr. Gross that fibrous polypus occasionally "springs from the septum" of the nose, although that it ever does so is denied by some surgical writers. We have seen a small pyriform pedunculated growth of this kind growing from the anterior portion of the septum; and Bryant mentions, in his "Surgery," that he has removed a gelatinous polypus and from the septum also.

(To be continued.)

---

*Insanity, and Hospitals for the Insane in Ireland. Twenty-second Report of the District, &c., Asylums in Ireland, 1872. Presented to both Houses of Parliament. Dublin: Alexander Thom. 1873.*

AMONG the subjects adverted to by the inspectors in their Parliamentary Report on the Establishments for the Insane in Ireland in 1872, we find that of the separation of chronic and incurable cases from the recent and acute, who may reasonably be supposed to be curable. This measure would be desirable both as regards the

patients themselves and society at large, partly because it would conduce towards fixing in the public mind the view universally adopted in the profession, that insanity is properly a bodily, rather than a mental disease, and that as a disease it is to be regarded and treated; that our asylums are not prisons for the detention of the mentally afflicted, but hospitals for their treatment. With the adoption of this view by the public we may expect to see a much earlier recourse to such treatment, particularly among the higher classes, with corresponding advantage both to the individual and society; while, at the same time, the burden of distress experienced by the friends and families of persons mentally affected will be proportionately relieved, a burden which now exceeds that experienced in the case of other hereditary diseases, and does so simply for this reason, that insanity is regarded, not as a disease of the body, but in another and quite a peculiar category.

Another point which the inspectors touch on is the undoubted increase in the proportion of the insane to the population. This actual increase, taken in connexion with the prospective increase due to the hereditary character of the disease, presents a highly alarming consideration to the view of the social economist. It is remarkable that this increase presents itself more among the lower and uneducated classes, who are less affected by the pace of the present age, than among those of the higher ranks, who participate in the high pressure life and work of the nineteenth century. But we find that this increase in the numbers of the insane runs strangely parallel with the increase in the revenue derived from the excise department, and bearing in mind the well-known tendency of indulgence in ardent liquors to produce insanity, we cannot doubt that we have here the *vera causa* of the portentous increase of this disease in the present day. And we must remember that it is not the drunkard alone who suffers, but that habits of intoxication are the most common starting-point for the establishment of hereditary neuroses, all tending to render the individual a burden to society, and extending their dire effects to the third and fourth generation, when nature herself generally interferes, and forbids the further propagation of the degraded stock.

It must shortly become a question of the utmost magnitude how far society will permit the profits of the distiller and the publican to entail an equivalent pecuniary loss, and an immeasurable social, moral, and intellectual loss, on itself.

We have next some reference to the question of moral insanity

and moral responsibility. It can hardly be questioned by those practically familiar with the daily life of the insane, that there still remains with many of them a knowledge of right and wrong, and a sense of moral responsibility; were it not for this, indeed, the working of our asylums would be very much more difficult than it is. In the worst stages of the disease the moral sense is, no doubt, destroyed, yet we would be inclined to pronounce it to be, as a rule, the last element of man's superiority that insanity touches. There are undoubtedly cases on record in which the moral sense has appeared to be destroyed, while the intellectual powers remained unaffected, and the question then arises, whether the individual is insane, or merely deeply criminal. We could hardly expect that it would occur to the legal mind, but we are surprised that it has not been instantly and fully recognized by the medical mind, that this question in such cases is to be decided, not by a reference to intellectual conditions, but to pathological signs and symptoms; in short, by a consideration of the presence of insanity, regarded, not as a disease of the mind, but as a disease of the body. In any case in which we might find the known physical signs of insanity present, the peculiar facies, the remarkable and characteristic odour, &c., there we would be entitled to predicate insanity, whether manifested in the moral or the intellectual faculties; but in none other would we exculpate a person charged with crime, on the theory of moral insanity apart from derangement of the intellectual faculties. It is unfortunate that so few physical signs or symptoms of this disease should as yet have been defined. But, at least, we have thus clearly indicated to us the direction our future researches must take, in order that they may be attended with practical results and real progress.

The inspectors have made an important suggestion with regard to the salaries of the physicians superintendent. The duties entrusted to these officers in Ireland far exceed, in importance and onerousness, those of the physicians superintendent in England, since the entire administration and financial management is placed in their hands in Ireland, where, however, in only four asylums, out of twenty-three, has the physician a resident-assistant to aid him in the discharge of his duties, whereas in the English and Scotch asylums such assistance is the rule, and by no means limited to one physician assistant, but two, three, four—nay, even six will be found in a single asylum. The Richmond Asylum, in Dublin, has accommodation for over 1,000 patients, and 980 were actually

resident on 31st December, 1872, yet the physician superintendent has only one assistant medical officer and an apothecary to aid him in the discharge of his laborious duties, medical and administrative. Yet we find the salaries of the Irish physicians to be very much below those of the same officers in the English establishments. In no case, except in the Richmond, or Metropolitan, Asylum above-named, does the salary reach £600, while £350 to £400 is the more usual amount. True, this is very good for a young man who may be fortunate enough to obtain an asylum appointment after three or four years of professional life, and one or two such there are; but in ordinary practice these same men, in the course of years, would be sure to reach a much higher standard of professional earnings. The inspectors accordingly propose that a commensurate advantage should be accorded to men who have spent many years in the service of the department, irrespective of the size of the asylum which they have been called on to administer.

They propose that with length of service and increase of experience a corresponding increase of salary shall take place, and undoubtedly this is but reasonable, and a step in the right direction. But, even apart from this question, it cannot be doubted, that as money falls in value, or purchasing power, so fixed salaries, such as that of asylum officials, ought to be revised and augmented, and this as a mere matter of justice.—First, because that if salaries remain nominally fixed, their actual value is lower, their purchasing power has decreased, and thus a man absolutely does not receive the return he counted on from his appointment. His professional brethren in practice slightly raise their fees to a commensurate amount, but his salary remains fixed, *i.e.*, really becomes lower. Secondly, the tax-paying public actually pay less, since they receive higher returns for their produce of all descriptions, in consequence of the decreased purchasing power of money, for it is the producer who receives the full and final benefit of all such change. Hence the farmer and the landowner actually pay less in county cess and other taxes, while the nominal amount remains the same, so that to augment official salaries is no more than justice to the officer, and is no injustice, or even hardship, to the tax-payer.



*Maclachlan and Stewart's Pathological Charts. No. 1. The Brain.*

Edinburgh: Maclachlan and Stewart. London: R. Hardwicke.

THESE charts promise to be as useful as they are novel. They are apparently edited by Dr. James C. Howden and Dr. J. Batty Tuke. No. 1 consists of a series of twenty-six diagrams of the brain, taken from the last edition of Quain's Anatomy, and Professor Turner's paper on the Convolutions of the Human Cerebrum. They represent the brain in different positions and at various stages of dissection, and are intended to aid the pathologist in the accurate noting of the position of lesions of the brain or its membranes; the seat of apoplexies, abscesses, &c., being traced on the diagrams in colours by the observer. The right hemisphere of the brain is in doubtful cases more deeply shaded than the left. As the cost of each chart is only 6d., we anticipate a great demand for this useful novelty.

*A Treatise on the Continued Fevers of Great Britain.* By CHARLES MURCHISON, M.D., LL.D., F.R.S.; Vice-President and Consulting Physician, London Fever Hospital, &c. Second Edition. London: Longmans, Green, & Co. 1873. 8vo., pp. 729.

ELEVEN years have elapsed since the appearance of the first edition of this magnificent work stamped its author as one of the foremost clinical observers and ablest writers on fever in this or any other country.

During the interval, Dr. Murchison has not been idle, for the present edition contains statistical tables, based on observations of 28,863 cases of fever admitted into the London Fever Hospital, in the twenty-three years ending 1870, while similar tables in the first edition were based on only 6,703 cases. Recent epidemics of typhus and relapsing fever have been diligently studied in the wards of the invaluable institution just named, and the modern bibliography of the subject, in many languages, has been thoroughly investigated and analysed—the result being the production of a treatise more elaborate, exhaustive, and authoritative, than any hitherto published on that group of diseases included under the comprehensive term, “Continued Fever.”

In the 36th volume of this Journal (for August and November, 1863, page 169), a very full analytical review of the first edition of Dr. Murchison's work appeared. That analysis will render it

unnecessary to treat the present edition in a similar manner—the more so as this is a book which should be in the hands of every practical physician. On the present occasion we would prefer to make a few remarks which have been suggested by a profitable and, we trust, a careful perusal of the work. We would merely mention that, in the present edition, the great question of temperature in fever is admirably handled and illustrated; the beautiful coloured plates of the eruptions of the continued fevers have been reproduced; the number of diagrams and woodcuts has been considerably increased; and the clear type, so characteristic a feature of the first edition, has been preserved. The “*errata*” are remarkably few in number, but the persistent wrong spelling of the word “*hæmatemesis*,” about pages 194 and 207, has much the same effect on the critical eye as a grain of sand beneath the conjunctiva.

The subjects to which our attention has, more especially, been directed, are briefly the following:—

First—The etiology of typhus and enteric fevers.

Secondly—The influence of season upon the prevalence of these diseases.

Thirdly—The nature and importance of the secondary affections of fevers in general, and

Fourthly—Its prophylactic treatment.

I. Perhaps no part of Dr. Murchison's book is handled with more consummate skill than that which deals with the great question of the etiology of Continued Fevers. For a concise *résumé* of the etiology of typhus, we must refer our readers to page 118; to page 496 for the etiology of typhoid, and to pages 337 and 338 for that of relapsing fever. As is well known, the most potent *exciting* cause in the case of each form may be expressed by a single word—thus the relation of typhus to *overcrowding*, of relapsing fever to *destitution*, of enteric to *decomposition* [of fecal matter], is now generally admitted. No doubt instances are occasionally met with in which the determination of the causes of an outbreak of fever is attended by no little difficulty; and others, again, in which both typhus and typhoid seem to arise simultaneously, and under precisely similar conditions. But in almost every case a careful investigation will solve the mystery. In the *Irish Hospital Gazette* (Vol. i., Nos. 2 and 3) a remarkable series of cases of typhus, enteric, and mixed typhus and enteric, is recorded by Dr. Grimshaw, who seems to have taken considerable pains to inquire into the circumstances under which this outbreak took place. The sufferers—eleven in number, and all

members of the same family—inhabited the parlours of a house in a back-street in Dublin. The rooms were over cellars into which the drainage of a back yard had found its way, converting them into a very offensive cesspool. This was in autumn, when enteric fever chiefly prevails. Every member of the family suffered; the three first cases were of the enteric type, but two of them presented the characteristic rashes of both typhus and enteric. The next three cases were all typhus. The seventh was of a mixed type. The eighth was enteric. The ninth, tenth, and eleventh patients suffered from pure typhus. The outbreak extended over a period of some two months.

Now, at first sight, it would appear that both typhus and enteric fever arose in this instance under the influence of one and the same exciting cause—the defective sanitary condition of the residence of the ill-fated family. But it is much more rational to conclude that the origin of the typhus fever is to be traced to the crowding together of eleven persons, in deteriorated health, owing to the surroundings, in moderate-sized and ill-ventilated rooms. It is especially worthy [of note that a period of cold weather preceded, by about ten or twelve days, the occurrence of the first case. There can be little doubt that the lessened ventilation, consequent upon the fall of temperature, had an important influence for evil.

One of the most convincing arguments advanced in support of the origin and spread of enteric fever from “*faecal miasm*” is, that this form of disease has increased uninterruptedly since the introduction of water-closets and house drainage. Dr. Murchison, speaking of enteric fever, says (page 444):—

“The admissions into the Royal Infirmary, since 1862, show that the disease is now endemic in Edinburgh as it is in London. It is to be noted that this increase of enteric fever in Edinburgh followed the introduction of new sanitary arrangements—the substitution for the scavenger and night-men of drains opening into the interior of the houses, but with a water-supply insufficient to prevent the escape of sewer emanations.”

Having regard to the comparative neglect of sanitary engineering in most private houses until very recently, the only wonder is that enteric fever has not devastated the length and breadth of the land. In the *British Medical Journal*, November 8, 1873, will be found a diagrammatic representation of the drainage system of a house in Harley-street, London, which was lately opened up by Mr. William Eassie, C.E. In the house alone *forty loads of black sewage deposit*

and foul refuse had to be carted away from a system of superficial drains and cesspools, constructed in the dark ages of thirty years ago. A case of the same kind came under our own observation not long since. The drainage of a house in a good street in Dublin had been suspected for some time. A case of enteric fever proved fatal in the house, and palliative measures were adopted. But, in a few years, the occurrence of a second case of the same disease brought matters to a crisis—the drains were ripped open, and the following state of affairs was discovered:—Commencing beneath a butler's pantry, in the rear of the house, a shallow rectangular brick-drain, covered merely by flags, wound its zig-zag course through the entire house to the coal-vault in front. There was little or no fall, and when opened the drain was found completely filled with black fœtid filth. The bricks were in places decayed, and there was reason to believe that, in addition to the direct escape of gas through the interstices in the flagging of the basement storey, the soil surrounding the drain had become impregnated with decomposing sewage matter. What would Professor Pettenkofer think of the "ground-air" of such a house?

II. Intimately connected with the question of the etiology of fever, is that of the influence of season upon its various forms. Season, in fact, is a notable *pre-disposing* cause in the case of both typhus and enteric fever, but more remarkably so as regards the last-named disease. As Dr. Murchison shows, typhus prevails *as an epidemic* irrespectively of season; but it may fairly be asked, would not summer epidemics prove much more severe, were it not for the very reason that they prevail at the warm season of the year? Certain it is that, apart from epidemic influences, typhus is essentially a disease of winter and spring. An analysis of the Registrar-General's (of Ireland) weekly returns of births and deaths in Dublin, for the years 1869–72 (inclusive), shows that the mean number of deaths from typhus in that city during the first twelve weeks of the year is 52, whereas the corresponding number for twelve weeks, six months later, is only 35. In other words, the mortality from typhus in summer is only about 67 per cent. of that in winter. This seasonal prevalence of typhus, in the words of Dr. Murchison, "is not referable to mere cold, but is more probably owing to the protracted overcrowding and more defective ventilation of the dwellings of the poor, during the cold weather." Another very evident reason is not mentioned by our author—the greater tendency to bronchial and pulmonary secondary affections in winter. Any-



one who has laboured in fever wards will remember with what apprehension he has watched the setting-in of a cold wet period, or a season of intense frost. The bronchial affection may previously have been easily managed, but the change of weather will speedily render it an unmanageable and eminently fatal complication.

When considering the factor "season" as a predisponent of enteric fever, Dr. Murchison says (page 445):—"Unlike typhus, enteric fever varies greatly in its prevalence according to the months and seasons of the year." Now, we take exception to the words "unlike typhus" in this sentence—at least, unless they are qualified. From what has been stated above it is quite clear that typhus also "varies greatly in its prevalence" according to season. We believe that the whole difference depends on the *epidemic* characters discovered by typhus, contrasted with the *endemic* nature of enteric fever. If typhus were merely an endemic disease, we should probably observe it yearly increasing in prevalence to reach a maximum in early spring, and then gradually dying down to a minimum at the close of summer. Conversely, were enteric fever more of an epidemic disease, it is likely that its annual development in autumn would not appear so marked, while outbreaks would occur more frequently than they do in late winter and spring. But this form of fever does not die down rapidly in winter. In Dublin it continues to be prevalent until late in spring. This agrees with Dr. Murchison's observations:—

"It is also worth noticing that the increased prevalence of enteric fever in autumn does not subside immediately on the advent of winter. . . . It would seem as if the cause of the disease were only exaggerated or called into action by the *protracted* heat of summer and autumn, and that it required the *protracted* cold of winter and spring to impair its activity, or to destroy it."

However this may be, the causes which operate in the case of typhus in winter must also influence the prevalence of enteric fever in this season—namely, overcrowding and diminished ventilation affording greater facilities for the spread of the disease by contagion, and lessening the vital powers of resistance. Pettenkofer's theory of "ground-air," also, must not be forgotten. Winter is the season when the higher temperature of the interior of houses would determine a current of air from without through the subsoil of the basement storeys.

On the topic under consideration, we have merely to add that the

abdominal secondary affection of enteric fever stands in much the same relation to heat as the thoracic affections of typhus do to winter cold—as Hippocrates<sup>a</sup> says: *Τῶν φύσεων, αἱ μὲν πρὸς θέρος· αἱ δὲ πρὸς χεῖματα, εὐ ἢ κακῶς πεφύκασιν.*

III. “Local symptoms,” “local morbid phenomena,” and “complications” of continued fever, are terms used by Dr. Murchison to denote what many generations of students at the Meath Hospital have been accustomed to hear described by Dr. Stokes as the “secondary affections” of fever. Ourselves pupils of this distinguished clinical teacher, who has so nobly and so well carried out the system introduced at the Meath Hospital by the illustrious Graves, we must plead guilty to a *penchant* for the latter term. But is it not really the best? In the first place, it precludes the introduction of any erroneous views as to the nature of fever. The doctrine of the *essentiality* of the fever state is maintained, and the true relation of the local symptoms to the essential malady is recognised and admitted. In the presence of the term “secondary affection,” there could have been no room for Broussais or his school; while the danger of treating the name of a local disease, rather than the patient, is once and for ever removed.

And this last point opens out an important question—the *nature* of these secondary affections. Dr. Stokes has maintained (and we think rightly), that in no case are they originally inflammations, and that where inflammatory processes are established they are a tertiary phenomenon arising from reactive irritation. The steps in the process seem to be—(1) Typhous infiltration or deposit; (2) intumescence; and (3) resolution or reactive irritation ending in inflammation. The occurrence of the bronchial affection Dr. Stokes would attribute to a process analogous to the softening of the heart, whereby the involuntary bronchial muscular fibre becomes softened and paralysed—the result being the accumulation of mucus in the tubes. If this view be well-founded, it is clearly improper to speak of bronchitis in fever. Again, in how many cases of enteric fever are there no evidences of inflammatory action in Peyer’s patches—only tumefaction, ending in resolution? On this point Dr. Murchison writes (p. 616):—

“The enlargement of the intestinal glands does not of necessity lead to ulceration. The morbid products, to which the enlargement is due, may

<sup>a</sup> Aphorisms. Section III.

be re-absorbed—absorption commencing about the tenth or twelfth day of the disease, and by the end of the third week being complete.”

But surely we may go a step further, and hold that cases of enteric or pythogenic fever may occur, unaccompanied by any morbid change in the agminated and solitary glands whatever. This, Dr. Murchison is not ready to admit. He says (p. 625):—

“The morbid appearances presented by the agminated and solitary glands of the ileum above described, *are constant in*, and peculiar to, enteric fever.”

The words, “constant in” are dangerous words, worthy of Broussais and the Symptomological School; and it is not easy to see how Dr. Murchison can reconcile the use of them with his definition of enteric fever, in which the essentiality of the disease is clearly asserted. No doubt it is difficult—nay, almost impossible, to prove either position; but, in a case of this kind, the argument from analogy may be adopted as a safe guide. By it we are taught that as we may have scarlatina *sine* eruptione, morbilli *sine* morbillis, typhus without maculæ, and typhoid itself without rose-spots—so, also, the glands of the ileum may, *in exceptional cases*, altogether escape in the presence of undoubted enteric fever. The very inconstancy observed in the degree to which these glands are affected in different cases, also leads, *à priori*, to the conclusion that they may sometimes remain perfectly healthy.

Turning to the head-affections of fever, the same remarks relative to their non-inflammatory nature will apply. Dr. Murchison especially alludes to the infrequency of cerebritis, or of arachnitis, in continued fever. Speaking of typhus, he says (p. 260):—

“The increased vascularity of the cerebral membranes in typhus must not be regarded as a sign of inflammation, and does not account for the cerebral symptoms observed during life. . . . The congestion, in fact, is mechanical or passive, never active. Moreover, I am satisfied from many observations that there is no relation between the vascularity of the membranes and the symptoms. *I have repeatedly known the most severe cerebral symptoms during life, without abnormal vascularity of the cerebral membranes after death.*”

Further on he speaks in much the same way of the brain itself. The correctness of these remarks will be endorsed by every accurate clinical observer of fever.

The importance of fully recognizing the non-inflammatory nature of the secondary affections is chiefly seen in relation to a wide subject on which we cannot enter here—namely, the *treatment of fever*. Suffice it to say, with Dr. Murchison, that “in the treatment of these complications we must be guided by general principles and by the symptoms in the individual case, never forgetting that *the primary disease has a tendency to induce great nervous prostration and depression of the heart’s action, which forbid all depleting or lowering measures.*” And again, that one of our chief objects should be “to maintain, as far as possible, the nutrition of the body, and stimulate, when necessary, the action of the heart by appropriate food and stimulants—taking care, at the same time, not to excite congestion or increase the work of the already over-tasked glandular organs.” Let us remember also Graves’ self-dictated epitaph:—“He fed fevers.”

IV. Although this review has already run to so great a length, we cannot conclude without calling attention to one other distinguished feature in Dr. Murchison’s work. We allude to the “Prophylactic Treatment of Fever,” a subject of vital interest, which has been dealt with in a masterly style.

There are still some persons in the ranks of the profession who cannot be brought to realise the importance of investigating the etiology of disease, and particularly of continued fever. “Oh!” say they, “it matters little whether this man owes his illness to overcrowding, destitution, foul smells, or two or all of these influences combined. There he lies, and our business is to cure him.” A short-sighted policy, truly! But there is a yet larger class who say, “Fever cannot be prevented.” All such persons we would refer, first, to Dr. Murchison’s observations on the Causation and Prophylaxis of Fever, and then to Dr. Murchison himself. Within the past few months he has been enabled, by accurate investigation, untiring perseverance, and wonderful acuteness—all aided by the experience figured forth in his treatise—to trace to its source one of the most mysterious and dangerous outbreaks of enteric fever which has occurred for many years. It would be impossible to forecast what results may follow from his labours in this instance. The propagation of disease, through the medium of milk or other food, opens out a wide field for study, and the revelations of the Marylebone Inquiry have directed the attention of everyone to Dr. Ballard’s observations, and to Dr. John Dougal’s experimental researches.



A noble future is dawning for preventive medicine, and the time is coming when, owing to the removal (in some cases), or lessening (in all cases), of its exciting causes, fever may be as uncommon in our country as the plague, the black-death, or any other "pestilence that walketh at noon-day."

---

1. *Clinical Lectures on Diseases of the Urinary Organs.* By SIR HENRY THOMPSON. Third edition. London: J. & A. Churchill. Fcap. 8vo, pp. 230.
2. *The Preventive Treatment of Calculous Disease and the Use of Solvent Remedies.* By SIR HENRY THOMPSON. London: J. & A. Churchill. Fcap. 8vo, pp. 72.

THE second edition of Sir Henry Thompson's admirable Lectures was noticed at length in this Journal, and now we feel it necessary only to repeat the high commendation which we then bestowed upon them. In them he deals briefly with the important subjects which he has discussed in full detail in his larger works. "His aim," he tells us, "has been to produce in the smallest possible compass an epitome of practical knowledge concerning the nature and treatment of the diseases which form the subject of the work," and admirably has he succeeded in doing this. Beginning with a chapter on the Diagnosis of the Surgical Diseases of the Urinary Organs, he treats afterwards of stricture, hypertrophy of the prostate, retention and extravasation of urine, stone, cystitis and prostatitis, diseases of the bladder, hæmaturia, and renal calculus. The present edition, with other new matter, contains a lecture on the early history of calculous disease, and the treatment best adapted for its prevention; and a reprint of this chapter, with one on the treatment of stone in the bladder by solvent remedies, constitutes the smaller of the works now before us. In the great majority of cases the preventive treatment of calculus has for its object the alteration of that state of the urine which produces the deposition of uric acid crystals within the kidney; and our object, as Sir Henry Thompson points out, is not to be attained by alkalies, which but temporarily relieve those who suffer from Cayenne pepper gravel, but by excluding, as rigidly as other conditions will permit, alcohol and saccharine and fatty matters from their dietary, and by correcting those errors in the primary digestion which are conventionally termed "defective action of the liver."

# PART III.

## MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

### TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

*Wednesday, December 10th, 1873.*

SIR DOMINIC CORRIGAN, Bart., M.P., M.D., in the Chair.

*Therapeutical Remedies recently introduced.* By WALTER GEORGE SMITH  
M.D., Fellow and Censor, K.Q.C.P.I.

OF the new drugs which have been introduced into medical practice within the last few years, it is remarkable how large a share has been contributed by organic chemistry, and it is worthy of note that several of them, although well known to professional chemists, remained for years objects of purely technical interest before there was any thought of investigating their physiological or therapeutical qualities. Having this origin, and carrying with them into medicine their strictly chemical titles, it is not surprising that the nomenclature of some of them, although really simple, should appear perplexing to those who have not kept abreast with modern chemistry. Some of them, moreover, are rare and of difficult production, and hence are so costly that, for the present, however valuable they promise to be, they scarcely come within the pale of general practice.

In the hope, therefore, that the exhibition of the more important of the new remedies and physiologically active agents might prove of interest to the members of this Society, I have ventured to bring them down this evening; and, without any pretensions to reading a paper, I will pass them briefly in review, and will point out their salient properties in a necessarily cursory way.

On glancing over the list of names, I find that they can be arranged appropriately under three heads:—

1. *Alkaloids and active principles*; guarana, methyl-strychnia, emetin, apomorphia, ergotin.

2. *Hydrocarbides and their derivatives*; xylol, bromoform, iodoform.
3. *Alcoholic compounds*; nitrite of amyl, bromal, chloral, croton-chloral, propylamine (so-called).

I.—*Guarana*.—This drug is an example of an old remedy re-introduced, for it was first noticed so far back as 1817. In connexion with its medicinal action, it is worth remembering that its active principle, the so-called *guarana*, is identical with the well-known *cafein* or *thein*, a base which occurs also in Paraguay tea (*Maté*) and in the kola-nut of West Central Africa. *Guarana* is practically nothing but a convenient channel for the administration of impure *cafein*. Curiously, the five genera in which *cafein* is known to exist belong each to a different natural order.

*Methyl-strychnia*.—This remarkable body,  $C_{21}H_{22}N_2O_2C_2H_5, H_2O$ , is not only interesting in itself, and as a specimen of a now numerous class of organic substances, but is more especially noticeable as an illustration of a recent and novel method of inquiry, which can scarcely fail to bear important fruit. I refer to the study of the connexion between chemical *constitution* and physiological action—a subject which has been so successfully prosecuted by various observers, notably by Crum Brown and Fraser, in Scotland, and by Pelissard, Jolyet, and Cahours, in France.

Brown and Fraser have shown that the *addition* of various organic radicals, *e.g.*, methyl and ethyl, to the molecule of some highly active bodies, such as the alkaloids, without destroying or altering the chemical integrity of the body, effects most extensive and unexpected changes in their physiological action. Iodide of methyl is inert in itself. For example, this very body, the hydrate of methyl-strychnium, produces a condition exactly the reverse of that produced by strychnia, and, in fact, instead of causing muscular spasms, due to increase of reflex excitability of the cord, it actually paralyses the peripheral terminations of the motor nerves, and it has accordingly been suggested as an antidote to strychnia-poisoning. 0.1 gr. of hydrochlorate of strychnia caused death in a rabbit in half an hour, while 30 grs. of iodide of methyl-strychnium (containing 21.1 grs. of strychnia), previously administered to the same rabbit, produced no effect whatever. In other words, more than 200 times the amount of strychnia can be administered with impunity in the form of the ammonium base than as any of the ordinary salts of this alkaloid. The ammonium bases of brucia, thebaia, codeia, morphia, nicotia, and atropia, have also been examined with the like startling results.

In the case of atropia, the poisonous activity of the base, instead of being so enormously diminished, is considerably increased, so that we cannot formulate any law as to quantitative alteration of toxic energy.

Interesting as these researches are, too extreme importance should not be attached to them, and they throw little light upon the question of the respective actions of the different alkaloids.

*Emetin*.—This substance is noticed in conjunction with the next, apomorphia, because they are the only two bodies which can be fitly employed subcutaneously as emetics. As in the case of ergot, we should distinguish pure emetin, *i.e.*, the alkaloid, 15s. per 3j., from the preparation sold in France under the name of “coloured emetin,” which is nothing but a brown extract, and is 12s. per oz. Pure emetin is at least three times as strong as the impure, and is recommended as a safe, rapid, easy, convenient, and agreeable form of emetic. The dose by the mouth is from  $\frac{1}{18}$ – $\frac{1}{6}$  gr., and subcutaneously, about  $\frac{1}{30}$  gr. (1 in 30 water, with a little sulphuric acid). Its solubility in water is 1 in 1,000. Its salts are uncrystallizable, and are very soluble in water.

*Apomorphia*.—The alkaloids are steadily and deservedly gaining confidence every day, and atropia, morphia, strychnia, quinia, &c., are counted our most trusty remedies. But hitherto we have been restricted to bases derived directly from the plant, and apomorphia is the first example of an alkaloid introduced into medicine which has been derived by decomposition from a pre-existing and well-known alkaloid, *i.e.*, it is a product, and not an educt.

The ordinary emetics are usually bulky in dose, uncertain in action, and are apt to produce depressing nausea and prostration, and apomorphia opportunely presents itself as a convenient and unirritating emetic, the one of all others which acts most speedily, and with least trouble to the patient. It is prepared by heating morphia for two or three hours with excess of hydrochloric acid; or, similarly from codeia, which is methylmorphia. The free base is unstable, and, originally white, turns green immediately on exposure to air. The hydrochlorate is more permanent, but even its watery solution begins to alter in a few minutes, and rapidly passes from an aquamarine tint to an emerald green colour, and ultimately becomes opaque. The physiological effects of the coloured solution are said to be similar to those of the recent solution, but it is less powerful, and is more irritating to the skin.  $\frac{1}{30}$ – $\frac{1}{20}$  gr. is a suitable dose for hypodermic injection. A solution of 1 gr. in 200 minims of water is a convenient formula. Occasionally giddiness, cerebral oppression, and præcordial anxiety have followed its use.

Apomorphia offers special advantages in dealing with refractory children, insane people, or with adults in some kinds of poisoning, when coma or delirium hinder oral administration. The only drawback is its costliness, and at present 3i. costs £1, *i.e.*, 4d. a grain.

In illustration of its effects I will mention two cases in which I have



employed it. Some time since I injected  $\frac{1}{30}$  gr. of apomorphia from a green solution into the arm of a woman at the Adelaide Hospital. In 10 minutes the stomach turned without causing her pain or distress. On December 6th I injected  $\frac{1}{20}$  gr. of apomorphia from a fresh solution into the arm of a patient suffering from spasm of the œsophagus, an affection to which he has been occasionally liable since an attack of cholera in 1867. He was of a peculiarly placid and unexcitable temperament. 5 minutes after the injection the pulse had risen from 70 to 104, and he complained of a sensation of heat in the head and of nausea, which rapidly increased. In 10 minutes he was inclined to be sick, but could eject nothing, and there was a sudden call to stool. The bowels were moved a second time in 5 minutes more; motions thin, copious, and very offensive. After the expiration of 20 minutes the pulse had fallen to 56; he was sleepy, pale, and felt and looked much prostrated. Within 2 hours he had completely recovered. No effect was produced on the œsophageal spasm, which was absolute and complete for a period of 122 hours, when the "stomach opened" after a long continuance of retching, and he was able to swallow some food.

Apomorphia is a striking instance of the assistance which scientific chemistry may, and does, lend to therapeutics, and it establishes another link between chemistry and medicine. Moreover it evinces the radical alteration produced in the physiological action of a substance by instituting an *apparently* slight change in its molecular constitution. In this particular case, the withdrawal of a molecule of water causes such a rearrangement of the atoms that we obtain new and unlooked-for results, for apomorphia,  $C_{17} H_{17} NO_2 =$  morphia,  $C_{17} H_{19} NO_3$ , minus water,  $H_2O$ .

*Ergotin*.—No better example than this can be brought forward of the confusion which results from the same name being applied to several distinct substances, or from a definite term being affixed to a body of unknown or uncertain composition. There are no less than 3 totally distinct substances included under the common designation *ergotin*, viz.:—Wiggers' ergotin (1831), Bonjean's ergotin (1840), and Wenzell's ergotin (1864).

1. *Wiggers' ergotin* is prepared by acting upon the spirituous extract of ergot, previously freed from fat, with water, whereby the reddish brown powder is left behind. It has a bitter taste, and is insoluble in water and ether, and difficultly soluble in spirit. Like Bonjean's preparation, it is an impure substance. In doses of from 3–7 grs. it gives rise to a nauseous bitter taste, followed by a confused feeling in the head, running on to headache, dilatation of the pupils, fall in the pulse, and abdominal pain; 7 grs. caused further a sensation of dryness in the throat, lasting for several hours. It costs 5s. per drachm.

2. *Bonjean's ergotin*.—Prepared by exhausting the watery extract of ergot with spirit of wine, and evaporating the spirituous solution. It is a brownish-red extract, with a peculiar odour of roast meat, and a sharply bitter taste. It is completely soluble in water and spirit, and is evidently a complex product. Its action appears to be similar to that of Wiggers' ergotin. Cost, 1s. 6d. per ounce.

3. *Wenzell's ergotin*.—According to Wenzell, ergot contains two alkaloids, ecbolin and ergotin, which can each be obtained as an amorphous, brown, feebly bitter powder, easily soluble in water and spirit, insoluble in ether and chloroform. The property of exciting contraction of the uterus is stated to reside in the ecbolin, but this statement needs confirmation. These bases seem to rest upon a slender foundation, their relation to Wiggers' and Bonjean's ergotins is not determined, and, in short, as matters stand at present, it is impossible to give a satisfactory answer to the question—what is the active principle of ergot?

II.—Of the second group little need be said—further than to point to the specimens on the table.

*Xylol*.—Had a very short-lived run in the treatment of small-pox, in which it was administered in capsules, in doses of 10–15 m. It belongs to the small but important group of bodies known in chemistry as the aromatic hydrocarbides, from the agreeable odour of some of their compounds, *e.g.*, essence of bitter almonds. The first term of this homologous series is the familiar benzine or benzol,  $C_6 H_6$ , and above this are five higher terms, the third of which is xylene or xylol,  $C_8 H_{10}$ .

Its name is derived from *ξύλον*—wood, because it was first found among the oils which are separated from crude wood spirit by the action of water.

When we consider the importance of the group of hydrocarbides to organic chemistry, and call to mind that they are hundreds in number, exist in every variety of form, and with every variety of properties, thus constituting in themselves a compact and symmetrical range of series, mutually connected by simple laws, it seems strange how little medicine has borrowed from them, for, with the exception of the terebene series, including oil of turpentine, and its numerous isomers, oil of juniper, oil of lemon, &c., it has not yielded much to the stores of therapeutics.

*Chloroform*,  $CHCl_3$ .—*Bromoform*,  $CHBr_3$ .—*Iodoform*,  $CHI_3$ .

These three bodies afford additional confirmation, were that needed, of the remarkable gradation of properties which exists between their halogen parents, Cl, Br, and I, and which extends throughout the majority of their combinations.

Thus Cl is a greenish-yellow gas.

Br is a red liquid.

I is a dark solid.

So we have—

$\text{CHCl}_3$ —a colourless liquid.

$\text{CHBr}_3$ —a reddish liquid.

$\text{CHI}_3$ —a sulphur-yellow solid.

*Bromoform* is less volatile than chloroform, more easily decomposed by alkalies, and appears to enjoy much the same properties as iodoform, but it is irritant, and does not appear likely to fulfil any useful indications in practice.

They are all prepared in an analogous manner. *Iodoform* contains 90% of iodine, and yet its taste is mild and non-corrosive. It is not new, for it was discovered in 1822, and has been long in use on the Continent. There is much evidence in its favour, and from its non-irritating action, ready absorption, and peculiar anæsthetic effect locally, it seems surprising that it has not come into more general use. Dose,  $\frac{1}{2}$ –1 gr.

III.—*Alcoholic Compounds*—*Nitrite of Amyl*.—This liquid is the second example only of an organic nitrite being utilized in medicine, the other being nitrite of ethyl—the essential constituent of the old and well-known “sweet spirit of nitre.” *Nitrate* of ethyl or *nitric* ether proper, is a heavier liquid than nitrite of ethyl, and does not appear well adapted for practice on account of its irritating tendency.

This amylic nitrous ether was discovered by Balard in 1844, more fully investigated by Guthrie in 1859, and again more thoroughly still by B. W. Richardson in 1863–64. We cannot doubt that we have here an agent of at least very remarkable and energetic powers, while it is noticeable that the morbid conditions in which the drug has been found serviceable were pointed out before it was used in practice. As a powerful and rapid modifier of the action of the heart, and as an extraordinarily active means of relaxing arterial tension, it stands alone; and with too scanty experience to pronounce positively on its precise powers for good or evil, we cannot mistake the prompt and effectual relief which it has in several cases given to the agony of angina pectoris and the paroxysms of spasmodic asthma. No doubt some serious consequences, such as troublesome headache and alarming prostration, have occurred after its use, but we should remember that much of the amyl nitrite, as sold, is very impure, and, in addition to not inconsiderable quantities of prussic acid, is liable to contain other organic compounds, *e.g.*, aldehyd and ethyl-amylic ether, which must interfere with its proper action. Unlike nitrite of ethyl, it does not appear to suffer decomposition by keeping.

*Bromal, Chloral, Croton-Chloral.*—Of these three bodies I may say, in brief, that ordinary or ethylic-chloral has secured for itself a firm position in medical practice; bromal offers little promise of utility; and croton-chloral is just entering upon its trial. They are all strictly analogous bodies, and are respectively the bromine or chlorine derivatives of aldehyds. Bromal is brominated ethylic (common) aldehyd; chloral is chlorinated aldehyd; and croton-chloral is a chlorinated derivative of crotonic aldehyd, *i.e.*, of the aldehyd of crotonic acid, a constituent of croton-oil.

*Bromal* is prepared in a manner similar to that used for chloral, *viz.*, by the action of bromine on alcohol. It is a colourless, oily liquid, sp. gr. 3.34, boiling above 212°, and, like chloral, forming with water a solid definite hydrate. It was discovered by Löwig in 1832. The hydrate =  $C_2HBr_3O, 2H_2O$  (Watts). When administered to the lower animals (frogs, rabbits, and guinea-pigs) in doses of from 2–15 grs., the principal symptoms produced are irritation of the air passages, restlessness followed by imperfect sleep, and anæsthesia, and finally dyspnœa and death, either with or without convulsions. It has been given to man in a few cases of disease, *viz.*, epilepsy, tabes dorsalis, and aortic valve disease, in doses of  $\frac{1}{2}$ –1½ gr. and upwards. It is soluble in 6 parts of water. It is evidently possessed of considerable activity, for 1 gr. has, in the human subject, produced unpleasant results, and 4 grs. have caused great depression and severe pain in the stomach. It seems unlikely to prove useful either as a hypnotic or as an anæsthetic. It is too irritant, and does not appear to offer any advantages as a therapeutic means over other safer and speedier remedies. It is stated to be barely supportable in an emulsion containing 1 in 300.

*Chloral.*—Of this now so familiar drug I would only say that it, as well as bromal, supplies us with a fresh warning to take heed not to push chemical analogies too far when dealing with the phenomena of life, and we shall only be led astray by regarding the stomach as a warm test-tube, in which the chemist can work out his problems as unerringly as in the laboratory. Because chloral is decomposed by strong alkalis, or their carbonates, in accordance with a well-known reaction, into chloroform and formiate of the base, the specious theory was promulgated that the action of chloral was thus simply explained, and that it, in fact, was nothing else than the action of nascent chloroform gradually evolved within the system.

Although this theory has met with, and still finds, general favour, it has, in my opinion, no valid support, and cogent chemical and physiological arguments can be arrayed against it. I think it should be held that chloral exercises a specific action of its own upon the organism, which is not to be reasoned out from an exclusively chemical basis.



It is interesting to recall that the discovery of chloral preceded that of chloroform, for it was in 1832, while studying chloral, which he had just discovered, that Liebig for the first time met with chloroform.

Other varieties of chloral, *e.g.*, amylic and propionic, are known to chemists, and probably an indefinite number of chlorals could be obtained by suitable means. Liebreich, the introducer of ordinary chloral, has suggested a new chloral, which is possessed of some remarkable properties.

To distinguish it from its predecessor, it is termed *croton-chloral*—*i.e.*, the chloral of the crotonic series. Dr. Hofmann found that some of the German chloral contained more or less of crotonic chloral, and its origin is accounted for in this way:—

The alcohol from which ordinary chloral is made in Berlin, by the action of dry chlorine on absolute alcohol, is purified by passing it over charcoal. Hence, by oxidation, some ethylic aldehyd is formed. This, when acted upon by the hydrochloric acid liberated in the reaction, suffers dehydration, and yields crotonic aldehyd— $2\text{C}_2\text{H}_4\text{O} - \text{H}_2\text{O} = \text{C}_4\text{H}_6\text{O}$ ; and this new aldehyd, acted upon by the chlorine gas, furnishes the new chloral,  $\text{C}_4\text{H}_3\text{Cl}_3\text{O}$ .

Most agents which act upon the nervous system affect it generally, or at least, engage in their sphere of action several important tracts. At the same time we possess some active drugs, which exhibit a remarkable determination to particular areas—*e.g.*, conium, curare, &c., and in this new compound, croton-chloral, we meet with a body which exerts a specialized and singular action upon a very limited nervous region.

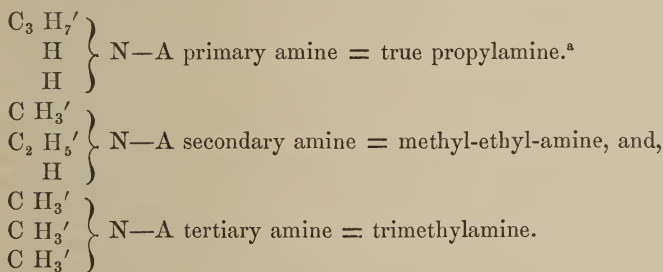
When administered to animals it produces, first, anæsthesia of the head, then loss of reflex movements throughout the body, and finally death by paralysis of the medulla oblongata. In man, however, when given in small doses it produces anæsthesia of the fifth nerve, and of that alone, while the sensibility of the body generally, and the pulse and respiration remain unaffected. This singling out of an individual nerve will, if confirmed, stand alone, and is a curious and suggestive phenomenon.

Hydrate of croton-chloral closely resembles in appearance the ordinary chloral. It may be prepared by passing chlorine gas into allylene, or by acting upon dichlorallyl and formic acid with alkalies.

*Propylamine* (so-called)—This, the last subject upon which I will offer any remarks, has given rise to some discussion and to much confusion in the medical journals, but it may be disposed of in a few words. The principal points which I would urge are the importance of precise nomenclature, especially in organic chemistry, and the necessity of discriminating clearly between isomeric compounds, or, in other words, not to mistake identity of *composition* for similarity of *constitution*. So far back

as 1817 it was announced that the fœtid odour of *chenopodium vulvaria*, a British plant, commonly known as stinking goosefoot, was due to the presence of a volatile ammoniacal compound ready formed—the first example of a free alkali in the vegetable kingdom. More than thirty years subsequently (1851) it was shown that the same volatile base existed in many other plants, and in various animals (maybug), its most abundant source being herring brine, and it was at first identified with the chemical compound propylamine, *i.e.*, propyl-ammonia. For years this was generally accepted, and the drug was introduced into medicine under that name by Dr. Awenarius, of St. Petersburg, in the treatment of rheumatism more especially. Even in the last edition (1865) of Guibert's excellent History of "New Remedies," this substance is treated of under the erroneous title of propylamine. But it has been known for some years (1862) that true propylamine has a different origin, and exhibits widely different properties, and it has been proved that the so-called propylamine of commerce, obtained from cod-liver oil, herring brine, ergot, &c., is in reality trimethylamine, with which it is isomeric.

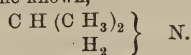
The ultimate composition of these bodies is  $C_3 H_9 N$ , but, of course, this formula in itself teaches us little, and we now know that there are at least three different bodies which own this empirical formula, and yet are totally distinct from each other. These bodies are—



We should remember, then, that as yet there is no such thing as real propylamine used in medicine, and what is styled so is undoubtedly trimethylamine.

The boiling point alone will distinguish trimethylamine from propylamine, for the former boils at  $40^\circ F.$ , and the latter at  $121^\circ$ . The dose of trimethylamine is 20 m. and upwards. It may be said, what is the use of this dissection of formulæ, this contention about names—let chemists fight their own battles. To this I would answer, not only is the confusion of names mischievous, but the properties

<sup>a</sup> There is also iso-propylamine known,



of these bodies are different one from another, and plainly all accurate knowledge is at an end unless names be used in their true signification. It is only because of their comparative novelty, and of the partial resemblance between propylamine and trimethylamine, that these two liquids have been confounded. Morphia and piperin are isomeric—their empirical formulæ are identical—yet no one dreams of confusing these two bases, for they are too widely apart in their origin and properties. In reference to the class of bodies to which trimethylamine belongs, viz., the amines (*i.e.*, organic ammonias), it may be observed, that, as all the natural alkaloids are probably either secondary or tertiary amines, it is only rational that we should look for active agents among such as can be artificially prepared.

If we now take a glance over the history of *organic* chemistry, *i.e.*, the chemistry of the hydrocarbons and their derivatives, from its birth, half a century back, we shall see that the principal services rendered by chemistry to therapeutics distribute themselves through several epochs. First, chemical analysis came to our aid, and revealed the existence of distinct active principles in drugs, and explained their composition. To this study we owe the whole group of alkaloids, and the knowledge of such facts, for example, that the properties of cherry laurel water, or of emulsion of bitter almonds, are entirely due to the production of prussic acid. The poisonous qualities of bitter almonds had been indicated from antiquity, and those of cherry laurel from the beginning of the eighteenth century.

Then, advancing beyond the mere separation of immediate principles, chemists submitted animal and vegetable matters to the action of the most varied and powerful re-agents, and thus, through the study of chemical metamorphoses, whole groups of new compounds were opened up, and novel, simple, and less costly processes for the preparation of bodies previously known were disclosed. In proof of this may be cited the present commercial manufacture of benzoic acid from hippuric acid, and of glycerin by the aqueous saponification of fats.

In pursuing these intricate studies chemists were naturally led, not only to the consideration of known substances in varied aspects, but also to the discovery of entirely new compounds.

How pregnant with value this line of research has been to medicine will be manifest when I mention among its offspring creosote, carbolic acid, collodion, chloroform, chloral, apomorphia, &c.; while in the arts we have from coal, benzine, aniline, with its splendid colour-derivatives, and anthracene, from which the colouring principle of madder is artificially obtained. In more recent times still physiologists and therapeutists, not content with passively accepting information from scientific chemists, assume, in their turn, the position of teachers, and by the happy combination of their joint labours we may justly look forward to an

array of discoveries, of which nitrite of amyl, chloral, and chloroform are the heralds.

In conclusion, I wish to say that my object in demonstrating these various drugs this evening was mainly on account of their possible novelty to some of the members, and to give them an opportunity of examining them at leisure. Some of the remedies were referred to chiefly from a chemical point of view, but I would not be understood as desirous of claiming too arrogant a position for chemistry in its relation to therapeutics, nor can therapeutics ever be built upon any one science, or be successfully studied without the co-operation of clinical observation.

But while I do not believe in the visions of those who see in the chemistry, and in the chemistry alone, of the future, a complete and harmonious system of therapeutics, the close and legitimate alliance which subsists between scientific chemistry and practical therapeutics cannot be too strongly upheld.

The CHAIRMAN said the Society must feel deeply indebted to Dr. Smith for this valuable and interesting paper. He should have been glad to make some observations upon it himself, but was not sufficiently acquainted with the subject to venture to do so. He hoped some of the members of the Society more familiar with the subject would favour them with some observations on the paper.

*Further Observations on the Use of Hemlock.* By HENRY KENNEDY, V.P. and Censor, College of Physicians; Physician to the Whitworth Hospital, Drumcondra.

In the course of last session I brought before the Surgical Society of Ireland a short paper on the Medical Uses of Hemlock. As the subject attracted some notice, and seems to me to be one of practical moment, I venture again to bring it under notice, and still more because a subject of this nature may very appropriately follow the interesting paper we have just heard from Dr. Walter Smith. In one which appeared in the *Practitioner*, some three years since, there was occasion to allude specially to hemlock, as being a drug, among many others, which, from whatever cause, had undeservedly fallen into disuse. It is certainly a curious question to ascertain why it is that medicines which had once gained a great repute, and no doubt possessed considerable powers, should fall into total oblivion. But so it is, as every one present is aware, and hemlock has shared this fate. I am not now, however, about to enter into the general question why some, or rather many, medicines should have gone out of fashion, as it were. To my own mind the reasons are obvious, and have been already given in the paper to which I have referred. I may observe, however, that this very disuse of potent drugs, and hemlock is certainly one of these, is a main cause why



therapeutics have not progressed as steadily as they would otherwise have done. If the labours of our predecessors be ignored or forgotten, how is our art to advance? If the knowledge of the present day be not utilized by our successors, can it be otherwise than that therapeutics must be stationary if, indeed, they do not retrograde? If we were to imagine the circulation of the blood, or any other equally well established fact in physiology, to be forgotten, it is clear that the science could not progress. And so it has happened with many potent and valuable drugs, the efficacy of which has been placed on as certain a basis as the nature of the subject admits, and quite enough to satisfy the demands of any reasonable mind. To some it will seem strange that I should venture to speak of therapeutics as having even an approach to as exact a science as physiology. I do not hesitate, however, to do so, for I have the strongest convictions of the efficacy of drugs, and I know that the virtues of many of them rest on a basis which it is not possible to shake. Nor can the fact of their having been ignored or forgotten alter the question. The facts remain even though the present age may forget them. Hence, for myself, I come to look upon therapeutics as having, in a degree at least, a strong claim to be considered an exact science. I mean, of course, as exact as the nature of the subject admits. I am the more anxious to bring this view of the matter prominently forward, for, as you are all aware, there prevails at the present time, and more particularly in London, much scepticism as to the virtues of drugs; and some even have stated their opinions openly on the point.

The remarks just made apply with particular force to the drug which I wish to bring under the notice of the Association this evening. Known for years as a potent medicine, and often used as a poison, hemlock has passed through the vicissitudes of other valuable drugs. About the middle of the last century, however, it seemed, chiefly from the writings of Stoerk, in Germany, to have reached a position from which it might be supposed it could scarcely have fallen. Yet, within a few years of the time of which I speak, the drug did fall into disuse, and though used occasionally since then, and frequently with extraordinary benefit, it has never regained the position it once held; and whether it will ever do so again, even with the assistance of the valuable labours of Harley, remains to be seen. But before making any further remarks of a general kind on this drug, the meeting will probably allow me to give the details of a few cases which were mainly treated by it, and in which its effects were very striking.

CASE I.—A young lady, of twenty-one, residing at Queenstown, Cork, was attacked with hoarseness and loss of voice. This persisted for several weeks, during which period all the remedies in common use were assiduously used, but without effect. As I had attended the patient

whilst she resided in Dublin, I was asked to suggest something, and I advised a trial of hemlock, which she got in the form of the succus conii. She at once improved, and in the course of a month the voice was quite restored, and the patient has remained well, some months having since elapsed. Dr. Cronin, of Queenstown, was cognizant of this case, and wrote me very lately that the patient continues well, and her voice natural, but that at times she feels it weak.

This was a marked example of the good effects of the hemlock, for it must have been observed that it was used after the ordinary remedies had failed. Need I add, too, how intractable many of these cases are, and more particularly after they have gone on for weeks.

CASE II.—A young woman of twenty-four, and married four months, was admitted into Dun's Hospital in August last. She had all the signs of high fever on her, and what might be described of the inflammatory type. I found, however, that the symptoms were due to an attack of inflammation of the left ovary. Pressure, even of the slightest, here caused exquisite pain; nor could she turn with any ease, or stretch down the leg of that side. Leeches, poultices, and salines were directed. To the former, however, the patient had such a repugnance that they were not put on. After some days, though the fever abated, yet the pain still remained, and she could not help herself without increasing it. In this state she was ordered hemlock, in the form of extract, and the effects, I must say, were very striking. Within twenty-four hours she was decidedly relieved, and in four days she was able to be up, and she left the hospital within the week.

CASE III.—A man of forty-six, of thin make, and who had been a compositor, but had given it up for some time, was attacked with severe pain in the abdomen. This occurred early in 1873, and was attributed by the patient himself to the lifting of a heavy weight. At first considerable intervals of time elapsed between these attacks; but as the summer wore on the attacks became much more frequent and severe, and finally he was obliged to give up his avocations. In this state he applied at the Whitworth Hospital, Drumcondra, in August last. Besides being thin, his look was now unhealthy. He stated that his bowels were very regular, and his appetite good; nor had he any trace of fever on him, nor blue line on his gums, which, as having been a compositor, he might have had. Just before my first seeing him he had had an attack of the pain, which lasted two days, and with great severity. Between times he said he was quite free of it. When in pain he described it as shooting across the abdomen, and often into the testes. On examining him as he lay on his back, the right rectus muscle was much more rigid than the left. By gentle rubbing this was overcome, and then a tumour, the size of a small apple, became very evident. This lay between the

liver and the cæcum, and when I made him inspire it clearly moved. Pressure on it gave pain, and the firmer the worse. It had no impulse. I must say I was and still am at a loss for the diagnosis of this case. Under the circumstances I put him on a course of hemlock, and within ten days he said he was better, and at the end of three weeks so well as to be able to resume his work. The tumour, I should say, seemed to me to lessen a little, but not to go away. I heard from this patient very recently, and he continues quite free of pain.

CASE IV.—A girl of twelve years of age was brought by her mother to the Whitworth Hospital, Drumcondra, for advice. The mother stated the child had kept her bed for three weeks previously owing to illness, and a glance was enough to show this had been the case, for the child was much reduced, had a rapid pulse, and sweated much every night. On examination there was found very general bronchitis through both lungs. The appetite was quite at a stand. Under the use of hemlock this patient rapidly regained flesh and strength, and, I need scarcely add, all the signs of hectic also passed away.

If it were necessary I could give a number of other cases, each of which presented some features of interest in itself. But those I have given are quite sufficient for my present purpose, and were selected to show the variety as well as the class of cases in which hemlock may with advantage be used. In what follows I will speak in a more general way of those affections in which it will be found of use. The first of these I would speak of is chronic rheumatism, known to all to be often so very obstinate. In very many of these cases, though not in all, hemlock acts peculiarly well. Thus, in one of the blind men from the Sackville-street Institution it seemed to do wonders. He was pale and sickly-looking when first seen, and suffered from severe pains, chiefly about the shoulders and scapulæ. In this instance, under the use of hemlock, he rapidly gained flesh, and got some colour in his cheeks; at the same time he lost his pains. The case, too, is the more striking, as many remedies had been tried with the man before I saw him. Again, in the case of a nurse, who was handed over to my care by my friend, Dr. Denham, the effects of hemlock were very striking. She got typhus fever, and had a very narrow escape for her life. When able to be up she got violent pains in her legs, which seemed to be of a rheumatic character. My hearers need not be told that, coming after fever, this state often proves both very severe and very obstinate. In this instance, however, under the use of the hemlock, the patient lost all her pains in the course of ten days. The patient herself, I may add, said it was the medicine cured her.

Another class of affections in which the hemlock may be used with

advantage are those of the chest, such as chronic bronchitis and phthisis. I have given it to a very large number of these cases, and, speaking generally of it, with benefit. Let me not, however, be misunderstood. It does not cure them. But it brings them into a state in which they are so much improved that they can leave hospital for the purpose of following their avocations; and it does this in a very short time. Even in cases of phthisis the benefit is generally very striking. I can safely say that two out of every three cases are, by its means, improved—that is, the disease lulls, if I may use such an expression. The patient feels better, and in some instances pulls up flesh; and this will occur without their coming into hospital at all. If they be in hospital, three weeks is, with my present experience, long enough to produce this favourable change. Indeed, if the drug do not cause some improvement in a fortnight, it may be given up. My friend, Dr. Grimshaw, is cognizant of a case in which, if phthisis do not exist, it has certainly been most seriously threatened, the disease being in the family. On two occasions this young gentleman has been at once bettered by the use of hemlock; and this while still living in the same locality. The patient himself, I may add, thought the improvement was due to the drug; and he is a medical student. Were it needed, a number of cases, where phthisis was already established, might have been detailed, and where the benefit was very striking. I am bound to say some cases did occur where no benefit seemed to arise; but they were exceptional cases. It is something gained, I believe, to know of a medicine which can effect so much; and, certainly, in my experience, with less risks or inconvenience than attends the use of cod-liver oil.

The last class of affections of which I would speak here are those of the bladder. In many of those where there is great frequency in passing water, or pain, or both, the hemlock will be found a very useful remedy. Of course, cases of calculi are here excluded. But leaving out such, my hearers know there are a great many cases met with where dysuria, in one form or other, becomes a real disease. In the first paper on the subject, a case of a boy of nine years of age was given, where both frequency and pain in passing water were very urgent symptoms. In it the hemlock acted very well and soon, and the boy remains well after two years. In cases, too, where persons are raised frequently at night to pass water, the hemlock seems to act very happily. In two such, where I had ordered the drug for chronic bronchitis, it not only relieved the chest, but it lessened the frequency of passing water, and to a very marked extent. I do not say it cured any, but it relieved in a way which made both patients most thankful. I cannot doubt it will be found a valuable agent in many of these cases.

It does not appear to me necessary to pursue this part of my subject further here. It seems to me enough has been advanced to prove that



hemlock is a drug of considerable powers, and in a great variety of cases, chiefly of chronic disease. It acts not only as a tonic, but as a marked anodyne; and in this respect its action is, as it were, double. The tonic effect is shown by the increase of appetite, by the improved appearance of the patients, and, frequently, by the gain in flesh; and its anodyne powers, by the way it allays irritability and relieves pain. In most strumous cases it acts particularly well.

And, now, with these facts before us, the question may be raised: Why has it fallen into disuse? Why as a remedy, as was said before, of great potency, and proved by a number of observers to be possessed of, at least, considerable remedial virtues—why has it been forgotten or ignored? The answer is very obvious. The conditions under which it was originally prescribed have not been preserved. But though several reasons showing how these conditions have lapsed might be given, I shall confine myself mainly to one, and this has reference to the dose in which the drug is now commonly given, and I hesitate not to say it is very much too small. I entirely agree with Harley, that unless the physiological action of the drug be produced, it will be next to useless as a remedy. Our predecessors used it in large and increasing doses, and the success which followed was remarkably striking. But at present the doses stated in the British Pharmacopœia are literally useless; the extract being put down as from two to six grains, and the succus from half a drachm to a drachm. As to the dose of the extract, I really cannot yet say what it ought to be. I may state, however, that I have given a scruple three times a day, and this when I knew it to be obtained at the best houses: and yet it did not cause any poisonous effects. The more ordinary dose has been fifteen grains, three times a day, so that I am forced to the conviction that,<sup>a</sup> from some cause or other, the extract is a very inferior preparation—a remark which has indeed been made by many before me. In passing I may observe the extract suits admirably for children; it goes well in a mixture, and can be made very palatable. As to the succus conii, there can be no doubt it is the most effectual preparation of the drug. It is unfortunate, however, that it should require so much spirit to make it keep. It may be given to an adult in two-drachm doses three times a day, and this without the slightest risk. I have given three-drachm doses, and much larger have been used. In passing I may observe I have given the powder, in the form of pill, in a large number of cases, and with marked benefit. This mode, however, though in use of old, has its drawbacks in the number which require to be given.

In conclusion, then, I would repeat that in hemlock we have a drug possessed of very considerable virtues, and that it would seem to differ from most others of an analogous kind in this respect—that it has two

<sup>a</sup> If I recollect rightly, the extract, used a century since, was made by the heat of the sun.

distinct powers, one being a tonic, and the other a sedative or anodyne action. The latter has been long known and often acted on, but the former does not seem to have been as generally recognized as its merits demand. To myself it appears that its tonic and restorative powers are of a higher order than its sedative action. But it certainly is possessed of both, and I must repeat, that in this last respect it seems to me a very valuable drug. If, however, any one determines on using it, let it, at any rate, get fair play. Let the dose and mode of giving it, so strenuously laid down by our ancestors, be strictly carried out, and I am much mistaken, indeed, if it do not prove a valuable remedial agent.

The CHAIRMAN wished to know from Dr. Kennedy what is the dose of the *succus conii*, or the extract, that he has found sufficient to produce the results referred to?

DR. KENNEDY replied, that in giving the *succus* he began with two drachms three times a day, and had frequently pressed it up to one ounce in the day. With respect to the extract, he considered there was not the slightest use in beginning with a dose of less than ten grains.

DR. AQUILLA SMITH asked whether any other treatment was used in the cases which Dr. Kennedy had detailed?

DR. KENNEDY replied that hemlock was exclusively used in the cases he had detailed. Some of the patients were in hospital, and that was an advantage.

DR. AQUILLA SMITH agreed with Dr. Kennedy that the doses which are affixed to the preparations of conium in the *Pharmacopœia* were much too small to produce any physiological action, and from the very beginning he was always opposed to the specified doses thus put forward. They were most objectionable, and calculated to mislead the junior portion of the profession. As to the large doses that Dr. Kennedy had given, such as, for instance, a scruple of the extract, it went far to show that the preparation of the extract was not very carefully carried out. As to Dr. Kennedy's inference, that all the cases he had detailed were cured by hemlock, Dr. Smith could not admit it. Neuralgic cases had a natural duration, and the recovery of the patient thus effected might have been a coincidence instead of a cure. In some cases the patients were impressed, Dr. Kennedy said, with the idea that it was this particular remedy which had cured them, but they all knew that this was a common notion on the part of patients, and that a great difficulty in the medical profession was to know how to distinguish between recovery and cure. Dr. Kennedy did not allude to the tincture of the seeds, which, in his (Dr. Smith's) opinion, was, probably, the best of all. There was a formula for it in the *Pharmacopœia*. A writer in America a short time ago showed that the most potent form in which conium could be used was the tincture of the fruit just before it arrived at maturity, and that

was the opinion which he (Dr. Smith) should be inclined to adopt. There were a great many analogous cases in other medicines, particularly the drugs derived from the same natural family as the conium; the seeds were the most active, and certain to yield the most fixed preparation of the drug. As used in general conium was absolutely inert. They were indebted, therefore, to Dr. Kennedy for drawing attention to the clinical study of such a drug. There was no doubt conium had a precise physiological action, and if practitioners pressed it until its physiological effects were produced, good results might be obtained, but he need not remind the Society that the physiological action of a remedy was widely different from its curative power. It must be admitted that the action of all these potent drugs was relative to the condition of the patient, and a physician might sometimes fall into error by relying on the physiological action of the drug. He thought Dr. Kennedy's cases—although they did not prove much as to the curative effects of conium—were sufficient to induce practitioners to give a fair trial to the drug, and to give it in sufficient quantities to produce its specific effects.

DR. HAWTREY BENSON, having been called on by Dr. Kennedy to state his experience, said that last year he had used the extract of conium in two or three cases, but as he had not the notes of them, he had not intended, until called upon, to speak on the subject. He had used the extract in three cases of enlarged glands. One, a boy twelve years old, had been in several hospitals, and under the care of other practitioners; but no advantage followed the ordinary treatment with iodine, cod-liver oil, and syrup of the iodide of iron. When the boy came under his (Dr. Benson's) care, he put him on doses of eight grains of the extract of hemlock three times a day, and the result was a most rapid improvement. He was one of those patients of whom it might be said that he was eaten up with scrofula. He had large glands, which were desperately ulcerated in the axilla and the groins. Under the administration of the conium he made a rapid improvement, gained flesh and colour, and the ulcers healed up almost entirely in three weeks. Dr. Benson was greatly struck with this, but in the two other cases the results were not so decided.

DR. KENNEDY said he had found hemlock very useful in cases of strumous ophthalmia. The first effect of the drug was to allay pain, and then, if there happened to be any running sore, it converted the flux from an unhealthy discharge into what was called laudable pus. As to the two cases Dr. Benson alluded to, it was possible if the drug had been pushed further the success would have been more marked than it was, but the result of the treatment in the third case was a striking confirmation of his views, and he thought enough had now been advanced to induce physicians not to lose sight of this drug in future.

The Society then adjourned.

# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

## THIRTY-SIXTH ANNUAL SESSION.

*Saturday, 13th December, 1873.*

LOMBE ATTHILL, M.D., V.P., in the Chair.

*The Preventive Treatment of Post-partum Hæmorrhage.* By A. H. M'CLINTOCK, M.D., &c., &c.

THE subject of *post-partum* hæmorrhage is one of such vast importance, and of such deep practical interest, that its introduction here can never be out of place. It is, moreover, one of those subjects on which every one of us must have had more or less experience, and, therefore, have somewhat to say about it. I am not going, however, to treat of it *in extenso*, but merely to consider the prophylactic measures which may be employed where we have reason to expect the occurrence of flooding, consequent upon the birth of the child.

There are certainly two, and probably three, conditions which influence the production of hæmorrhage after delivery; one of these, and by far the most important, is the muscular contractility of the womb. Another is the state of the circulation at the time of delivery. The more free the patient be from vascular excitement the less firm need be the amount, or degree, of contraction of the uterus that will suffice to resist the escape of blood from the utero-placental vessels. This must be self-evident, and yet recent writers on the anticipation and prevention of *post-partum* hæmorrhage take no notice of this element in the production of the flooding. A third condition there is, whose influence must not be altogether ignored, and that is the coagulable power of the blood itself. This property, I fully believe, plays some part, though probably a subordinate one, in arresting sanguineous discharges from the womb after labour, as well as at other times.

Keeping these fundamental principles before us, let us proceed: and first, with regard to the premonitory symptoms of the hæmorrhage in question. Vascular excitement towards the end of gestation and during labour, always forebodes hæmorrhage. Madame La Chapelle seems to have been well aware of this, but the author who lays most stress upon it, and has most ably pointed out and illustrated its influence, is Gooch. Hæmorrhage after delivery, attributable to this cause, Gooch describes as "a peculiar form of hæmorrhage;" but the correctness of this title



may justly be questioned, "for though it possesses some features which distinguish it from the ordinary attacks of flooding (solely referable to atony of the uterus), yet they are not sufficient to constitute any essential difference, or materially to affect the practice that is to be pursued for its suppression."<sup>a</sup>

I confess I always feel uncomfortable when I find the pulse permanently rapid and jerking towards the end of labour, especially if the uterine action be wanting in strength; and under such circumstances I endeavour, as far as time will permit, to adopt precautions against hæmorrhage, and to have every available resource in readiness to suppress it. I very well remember the late Dr. Labatt, a man of great experience and sagacity, impressing on me the importance of attending to this symptom after delivery. He said that whenever he found the pulse to range above 100 at this critical period, it led him to look out for flooding or convulsions, and to be in no hurry leaving the patient's house.

It very seldom happens we can foretell, during gestation, that the uterine contractions will be inefficient, except by the experience of the woman's past labours. The presence of any tumour in the uterus, however, might lead us to fear this result, and I have published a case of this kind where fatal hæmorrhage succeeded to delivery.<sup>b</sup> In like manner, unusual distension of the uterus from plurality of fœtuses, or from dropsy of the amnios, might awaken an apprehension in our minds that hæmorrhage *post-partum* would be apt to take place. Levret held precisely the same opinion, for he says that, on all occasions when we see a patient extremely large, we must carefully guard against a too rapid delivery; and he points out very clearly and distinctly how a sudden emptying of the uterus—as when the child and waters are discharged at the same time—favours the production of hæmorrhage.

In the progress of labour, and especially in the second stage, the character of the pains affords a very reliable indication as to the probability of hæmorrhage. This every accoucheur of any experience must have observed. Both Dr. Whittle and Dr. Atthill pointedly allude to this premonitory symptom. "The pains," writes Dr. Whittle, "are of this kind—they are strong and quick; they do not gradually culminate into a strong pain, and subside again, but they are sharp, quick, and cease almost suddenly; and the intervals between the pains are long in proportion to the length of the pains."<sup>c</sup> Such pains as these he regards as very sure forerunners of flooding, and in this I quite agree with him, and think Dr. Whittle has done good service in directing the attention of practitioners to so reliable and so obvious a precursory symptom of hæmorrhage. In a subsequent communication he tells us that the above

<sup>a</sup> M'Clintock and Hardy's Midwifery, p. 217.

<sup>b</sup> Clinical Memoirs on Diseases of Women, p. 116.

<sup>c</sup> Brit. Med. Jour., 27th Sept., 1873.

description was not intended for cases in which the uterus had become exhausted by prolonged labour, nor does he think the same mode of treatment would be at all applicable to the two cases. I have frequently had occasion to observe, and I am sure there are many here whose experience can corroborate what I am about to say, that extreme mental depression (whatever may be its cause) can exert a somewhat paralyzing influence upon the uterus. The free administration of chloroform, too, very often does the same, but not always, whilst there certainly are some women in whom this anæsthetic has quite the opposite effect: these latter patients, I have remarked, are keenly susceptible to pain of any kind, and the intense terror and agitation which the labour pains create in their minds prove a psychological cause of derangement in the function of the uterus (analogous to what may occur with regard to the functions of other organs of the body). Now, by the exhibition of chloroform this source of disturbance of the uterine action is at once removed. Except in these special cases, however, I would not use chloroform in any instance where there was reason to dread the occurrence of flooding.

It must be admitted that not a few cases of *post-partum* flooding present themselves without any warning whatsoever, and where consequently we could not have anticipated it unless by the experience of the woman's previous labours. If flooding followed delivery in any former confinement, it should then be our duty to adopt precautionary measures against it, and at the same time be prepared to meet it.

The prophylactic measures against *post-partum* hæmorrhage are based on the principles I have just endeavoured—though very briefly—to point out. It is always desirable that the circulation should be not only free from excitement, but, moreover, not in an excitable state when labour comes on. “That disturbance of the circulation,” writes Mr. Robertson, “plays an important part in uterine hæmorrhage, and that it consequently deserves the especial attention of practitioners, is most true.” In cases where the history of the patient's previous labours leads us to apprehend flooding, attention to the pulse is of paramount importance. To secure the desiderated quietude of the vascular system, all that is required in ordinary cases is open-air exercise, abstinence from stimulants, and regularity of the bowels; in addition to these means, we might give digitalis and cooling medicines; and in full plethoric persons, I have no doubt the abstraction of blood from the arm, as recommended very strongly by La Chapelle (and at one time commonly resorted to in the management of pregnancy), would be very serviceable. Although the use of the lancet is still out of favour—or rather out of fashion—I am one of those who believe it will yet regain its true place as one of the most potent of our therapeutic agents. To Dr. Gooch belongs the merit of directing the special attention of practitioners to the important part which the circulation plays in the production of *post-partum* flooding, but

I long ago expressed doubts of the propriety of styling the hæmorrhage where this symptom is prominent “a peculiar form of hæmorrhage,”<sup>a</sup> as it does not differ essentially from hæmorrhage the result of simple atony of the uterus, and, once it sets in, is to be treated on the same principles.

We occasionally meet with pregnant patients in whom rapidity of the circulation depends on causes quite the opposite of plethora or over-sanguification. Here a line of treatment, totally differing from that above described, must be pursued.

Where the premonitory symptoms, or the result of previous labours, furnish grounds for expecting hæmorrhage, there are two means which should be employed in addition to the slow extraction of the fœtus, and following down of the uterus with the hand, &c. These two are, letting off the liquor amnii by artificial rupture of the membranes, and the administration of ergot of rye. That the discharge of the waters early in the second stage increases the energy of the pains, and favours the tonic contraction of the uterus after its contents have been expelled, not only coincides with every-day experience, but is in accordance with the well-established law of uterine contraction, that to be permanent and enduring it must be gradual and not sudden. The principle, then, on which this practice rests, is perfectly clear and rational, and the practice itself has been recommended by many obstetric writers, some of them of the highest eminence. Both the principle and the practice deduced from it were clearly and fully described by Levret over 110 years ago. Dr. Robert Lee, in his *Lectures upon Midwifery*, published in 1839 (in *London Medical Gazette*), very strongly advocates rupturing the membranes early in labour where we have reason to fear *post-partum* hæmorrhage, and he narrates some striking examples of the good effects of the measure. That so comprehensive a writer as Dr. Churchill should make no mention of the practice in question appears to me very strange, and supplies some palliation for the complete silence of Dr. Whittle and Dr. Atthill on the same point. The time to select for this puncture of the membranes is when the os is nearly fully dilated—the presentation, of course, being known to be a head or pelvic extremity. It is important for the success of the measure that the waters drain off, and to aid in this object it may be requisite, as Lee points out, to push up the head during a pain.

Where hæmorrhage after delivery is threatened, Levret advises the patient to be restricted to a lying posture from the beginning of the labour, in order, as he says, to guard against acceleration of the process; but another advantage from this precaution, which Dr. Dewees pointed out, is that it tends to keep the circulation more tranquil. Denman gives quite the opposite advice. He writes:—“When from former

<sup>a</sup> M‘Clintock and Hardy’s *Midwifery*, p. 217.

events there is reason to be apprehensive of hæmorrhage subsequent to the exclusion of the placenta, that has been altogether prevented, or very much lessened by delaying the time of the patient's going to her bed till the child was upon the point of being born, or even suffering it to be born while the woman sat upon the lap of one of her attendants." Great though my respect is for the authority of Denman, still I must candidly admit he leaves himself open to the severe but just criticism which Dr. Dewees pronounces on this piece of advice:—"Now," Dr. Dewees writes, "we would ask any one at all conversant with the economy of the uterus during and after labour, how an erect position, and the sudden evacuation of the waters at the moment the child was about to be born, can possibly contribute to the only circumstance at all available in the case under consideration—namely, the permanent contraction of the uterus? In the first place, an erect position will always be attended with a quicker circulation than a recumbent one, it will permit the waters to escape with more suddenness and rapidity than a horizontal and, consequently, the risk of atony must be increased."

In Dr. Hardy's and my "Midwifery," we devote a few pages to the consideration of the "prevention of hæmorrhage after delivery;" and, having noticed the recommendation of Dewees, to "rupture the membranes as soon as the labour is active, and the os uteri sufficiently dilated, or easily dilatable," we go on to say—"as regards breaking the membranes, we cannot speak from experience. The proposal, certainly, seems a rational one, and well calculated to promote the object in view, but should not be acted upon, we think, without mature consideration, and taking all the circumstances of the case into account: it has, however, the sanction of Dr. Lee to recommend it." It is twenty-six years since I penned the passage just quoted, and I *now* can say that I have adopted the precaution there described on very many occasions, and am fully persuaded it is a most valuable, and always a feasible, auxiliary in the prevention of flooding after delivery; and Dr. Dewees, from "many years of experience," was convinced it is the principal means to be relied on for preventing hæmorrhage.

Of all the resources, however, against *post-partum* flooding, I believe the most effectual to be ergot of rye. The possibility of the ergot exerting some hurtful influence on the child need not deter us from its employment in these cases, for if the ergot fail to excite uterine contractions, the child will most assuredly be no way influenced by it;<sup>a</sup> and if the drug produce the desired effect on the uterine muscles, delivery will in most cases take place before danger can arise to the child—and if not,

<sup>a</sup> That the action of ergot on the foetus is mechanical and not physiological, I have endeavoured to show in a paper read before this Society, and published in *Dub. Quar. Jour.*, May, 1865, p. 484.



we have the alternative measure of the forceps, which can safely be resorted to.

Who first employed ergot for the purpose of averting hæmorrhage, I cannot say. It seems highly probable that, soon after the peculiar properties of the drug became known to accoucheurs, it would be so used. I find Dr. Dewees gave it with this intention in a case related in the fourth edition of his "*Midwifery*," published in the year 1830.

When I was assistant to Dr. Charles Johnson, at the Lying-in Hospital, I frequently saw ergot given as a preventive of hæmorrhage. It used to be administered at one of three periods, viz., when the head was on the perinæum, or immediately after it had cleared the vulva, or after the expulsion of the fœtus, and as soon as the insertion of the cord into the placenta could be felt.

"By giving ergot before the child has been expelled," writes Dr. Hardy,<sup>a</sup> "some time may be gained; but, should the placenta be morbidly adhering to the uterus, the difficulty of introducing the hand for its removal will be greatly increased. By adopting the third plan, this source of apprehension is avoided. To this method it may be objected that much time will, perhaps, elapse, and a considerable quantity of blood be lost, before the ergot is administered; nevertheless, the possibility of the placenta being morbidly adherent should be ever present in the mind of the practitioner, and deter him from resorting to a measure which may so greatly augment the danger of the complication." Thus wrote Dr. Hardy in 1845, and the opinions therein expressed I held in common with him. But all my later experience has convinced me that, to be of real service, the ergot must be given some little time before delivery; and, also, that the objection he advances against this mode is practically of no weight, inasmuch as morbid adhesion of the placenta is a very rare occurrence. Dr. Whittle's plan is to administer, as soon as the os uteri is fully dilated, a full dose (that is, one teaspoonful) of a liquid extract of ergot twice the strength of that of the Pharmacopœia. This is exactly equivalent in strength to what I myself give, viz., two drachms of the liquid extract of the British Pharmacopœia—a preparation I have used for some years back to the exclusion of all others, and which very seldom fails to produce the specific effects of the medicine on the uterus. In dealing with primiparæ, Dr. Whittle very properly cautions us not to administer ergot until the soft parts are pretty well dilated, as well as the os uteri; and to give the drug in much smaller doses, as it sometimes acts with unusual energy in primiparous women.

In a paper published, May, 1846, the late Dr. Thomas E. Beatty—so well known and respected in this Society—relates his experience and his impressions as to the value of ergot under the particular circumstances we

<sup>a</sup> Dub. Quarterly Journal, May, 1845.

are now considering, and he states he had been in the habit of administering *secale cornutum* "immediately upon the birth of the child, and before hæmorrhage takes place." On analysing his cases, I find that in five, out of the seven which he details, the medicine was actually given some twenty or thirty minutes *before* the expulsion of the fœtal head; so that it is fair to assume his more usual practice was not to wait for the child to be born before administering the prophylactic. His concluding remarks are so apposite that I must be allowed to borrow them:—"The cases I have adduced are, I think, sufficient to show the value of the practice I would wish to recommend. They are, in my mind, convincing proofs of the efficacy of the *secale cornutum* as a means of preventing one of the most formidable evils we encounter in obstetric practice. Indeed, my confidence in it is so great that I now fearlessly undertake the management of cases which, without such aid, we all dread to encounter. It appears to me," he continues, "that the ergot prevents uterine hæmorrhage after delivery in two ways; first, by inducing a complete and permanent contraction of the uterine fibres, thus causing constriction of the blood vessels; and, secondly, by diminishing the force and frequency of the heart's action, and thus rendering the effusion of blood less impetuous and more easily restrained. In all cases where this medicine is given in a full dose, it has the effect of moderating the action of the heart." This lowering effect of ergot upon the pulse had previously been noticed by Dr. Hardy, in the paper from which I have already quoted, and no doubt it contributes, as Dr. Beatty points out, to the hæmostatic action of ergot on the uterus. In these cases of apprehended flooding, whilst it is most important to maintain a moderate compression of the uterus with the hand, it is, at the same time, desirable that we should not be in any hurry to press off the placenta; but wait for ten or twenty minutes, so as to give the uterus time to recover from the strong efforts required to propel the fœtus into the world. Should hæmorrhage come on in the mean time this rule may have to be departed from.

Dr. Atthill seems to avow himself an advocate for the forceps in preference to ergot as a means of averting hæmorrhage. Every one must admit that a patient will be less liable to flooding if delivered before her system is exhausted and the muscular irritability of the uterus worn out; but in the present day there is little danger of this happening, as the forceps is so frequently and so promptly resorted to in the management of labours, that any additional incentive to its early employment is assuredly quite superfluous. In point of fact, it is not after tedious labours that hæmorrhage is most apt to occur, but rather in those where there is little resistance to the expulsion of the child, and where, consequently, the second stage is brief in duration. The short, inert pains which prognosticate hæmorrhage, arise from what we may call idiopathic atony of the womb; and here the use of the forceps, without previous stimulation of the

uterus, would be directly calculated to induce the very danger we would avert; whereas, if we stimulate the torpid uterus first (by rupturing the membranes and by ergot), there will rarely be any need for a subsequent recourse to the "iron hand."

In a former part of this communication I threw out the suggestion that some deficiency in the coagulating property of the blood might probably be a pre-disposing cause of *post-partum* flooding. On this principle, whether it be correct or not, I have sometimes given gallic acid for days or weeks previously to the setting-in of labour, and have reason to think well of the practice. In the same way, I think, we are to explain the good effects which Dr. Bassett (of Birmingham), attributes to a course of iron. He writes (*Brit. Med. Jour.*, 22nd Nov., 1873):—"After an active experience, extending over five-and-twenty years, and a very careful examination of all the circumstances surrounding *post-partum* hæmorrhage, I have arrived at the conclusion that the best method of anticipating it is to prepare the patient for her confinement by a course of medical treatment extending over a period of from four to six weeks, the basis of such treatment being the administration of iron."

In the way of preparative treatment of this kind, Denman says that in those who have suffered from hæmorrhage in their former labours, he "has recommended their taking some tonic medicine, as one grain of zincum vitriolatum two or three times a-day for several weeks before the time of their delivery, and the use of the cold bath throughout the latter period of pregnancy, even to the day of their delivery."

The VICE-PRESIDENT (DR. ATTHILL) said there were several points in Dr. M'Clintock's valuable and interesting paper which ought to be specially discussed. Dr. M'Clintock referred to the rate of the pulse as being, in addition to the peculiar pains observed by Dr. Whittle, a premonitory symptom of impending hæmorrhage. He (the Vice-President) had no hesitation in bearing testimony to the accuracy of this statement in a certain class of cases. The condition of the pulse was sometimes a very important indication. A quick pulse in labour occurred in connexion with two very different classes of patients—namely, in those who were of full plethoric habit and in those who were in an anæmic condition, with an easily-excited and easily-exhausted nervous system. Now, as far as his experience went, he had not seen that the quick pulse of a plethoric woman was an indication of *post-partum* hæmorrhage—in other words, he did not think that women of plethoric habit, in whom a quick pulse existed during labour, were more liable to hæmorrhage than other females. Possibly in these women the blood might possess a higher degree of coagulability than in women of a different constitution, but certainly he did not look on a quick pulse in an ordinary plethoric woman as an indication of any great importance.

On the other hand, when he met with a quick pulse in an anæmic woman of feeble muscular habit, he regarded it as an important index, but then he considered it as an indication of nervous exhaustion which, in the paper alluded to by Dr. M'Clintock, he (Dr. Atthill) had pointed out as a cause of *post-partum* hæmorrhage. The mental depression alluded to by Dr. M'Clintock was nearly always marked by a quick pulse, and was but another phase of nervous exhaustion. Dr. M'Clintock was mistaken in supposing that he (Dr. Atthill) recommended the forceps in preference to ergot in the class of cases under consideration. He was not aware that he had ever treated a patient in whom he anticipated *post-partum* hæmorrhage with the forceps alone. He invariably administered ergot first, and then, if necessary, delivered the patient with the forceps. He did not give ergot to cause the expulsion of the child. He gave it, as Dr. M'Clintock rightly laid down, for the purpose of stimulating the uterus to contract, and he was always prepared to use the forceps if delivery did not rapidly occur; and in these cases of exhaustion of the uterus, that was seldom the case. He thought the forceps a valuable aid to the ergot in these cases. He would take the liberty of quoting from his (Dr. Atthill's) paper, referred to by Dr. M'Clintock (*British Medical Journal*, 1st November, 1873):—"In fine, give ergot in cases of labour in which you suspect that *post-partum* hæmorrhage is likely to occur, but do not rely on it exclusively; when symptoms, indicating that the power of the uterus is flagging, show themselves, prevent the exhaustion becoming excessive by the use of the forceps; when you do apply them, use them as *aids* to the uterus, not as *substitutes* for its action." "Use the forceps *judiciously*, and you will seldom have any hæmorrhage. Here, however, I would protest, as I did at the meeting of the Association against the injudicious and indiscriminate use of the forceps. Judging from published returns, I believe that not a few practitioners apply the forceps simply to save time and to free themselves from an irksome delay. Lamentable consequences must certainly follow such a practice." These passages accurately expressed his (Dr. Atthill's) views. With respect to early rupture of the membranes, he wished to say that he never administered ergot without rupturing of the membranes previously. He had made this an invariable practice, without being aware of the recommendations of Drs. Hardy and M'Clintock to rupture the membranes as soon as the os was fully dilated, and he thought that was a practice which should always be carried out.

He (the Vice-President) had laid it down, in his paper "On the Anticipation of *Post-partum* Hæmorrhage," that the too rapid evacuation of the uterus, whether naturally or by injudicious extraction by the forceps, or a too rapid expulsion of the placenta, might produce *post-partum* hæmorrhage; but that case was different from the hæmorrhage caused by the exhaustion of a long labour. All of them had seen cases where the



uterus was exhausted even after a labour that had only lasted a few hours. There was one other point which had not been alluded to by Dr. M'Clintock, and that was, that *post-partum* hæmorrhage was often induced by the injudicious management of the third stage of labour. He believed that a large number of cases of *post-partum* hæmorrhage were caused by the too rapid extraction of the placenta, a practice which was too generally carried out, and to which he strongly objected. Dr. Denman, in his admirable work, stated he had tried experiments to see whether any harm would follow from leaving the placenta for a considerable time in the uterus. He found no unfavourable results to follow, and he laid it down as an axiom that the placenta might be left in for four hours. Four minutes would, Dr. Atthill thought, be nearer the time the placenta was now, in general, left in the uterus; but he considered a medium between the two extremes should be adopted. In his opinion no attempt should be made to remove the placenta for at least fifteen minutes after the expulsion of the child. Even that, he thought, was often too soon. He always kept his hand on the fundus until the afterbirth was expelled. Doubtless the pain that expelled the child frequently also detached the placenta, but it seldom expelled it, and he thought nature intended it to be left for a time in the uterus to cause that organ to contract. Many practitioners, for the sake of getting rid of the trouble of being kept at the bedside of the patient, removed the placenta immediately after the birth of the child. If this practice be adopted, it should be done by pressure and not by traction. Dr. Mathews Duncan pointed out that when traction was employed the placenta acted like a piston, and drew blood from the uterus.

DR. CHURCHILL said that perhaps the omission in his book arose from the fact that he took it for granted that the membranes either had ruptured, or had been ruptured at the beginning of the second stage. He thought, perhaps, Dr. M'Clintock meant that they should be ruptured a little earlier, before the first stage was completed. He thought there was a slight want of precision in speaking of the quick pulse, and he should like to insert a word in the paper—viz., “permanent quickness.” A great many years ago he read a paper before the Society upon the variations of the pulse during labour and after delivery; and he remarked then that whenever the pulse did not diminish in frequency after delivery, they might certainly look out for hæmorrhage. During the second stage the pulse is quickened during a pain, and then subsides; as the stage went on it subsided less, and went on quickening until the end. When the labour is over the pulse, which during the last bad pain might be 140, fell down to about its natural standard. Then, when reaction took place, it might rise again, to fall afterwards to its natural standard; but when it remained at 120, then they might anticipate danger. In all Dr.

M'Clintock said about ergot he agreed, and he had nothing to add to it. There was, however, another matter, although it did not quite come within the purview of Dr. M'Clintock's paper, which was rather the signs than the treatment of hæmorrhage, but which was not altogether alien to it, and that was when the patient had had hæmorrhage before, he always stood over the patient with the uterus grasped in his hand; and he found that he was able to control the hæmorrhage. He had one patient in whose successive labours he had to stand over her thus for two hours. Now he wanted to say a word in opposition to what the Chairman said as to the precipitate delivery of the afterbirth. Provided the Chairman's observations were confined to forcible abstraction by the cord, he quite agreed with him, but he did not agree that the placenta should not be extruded as soon as possible. For a great many years he had been in the habit, by firm pressure and grasp of the uterus, of making the uterus expel the placenta within five minutes, and he had never yet seen hæmorrhage follow. He had seen far more hæmorrhage follow the birth of the child when the placenta was not interfered with, or where the placenta had remained half an hour in the uterus before being taken away. He did not know that in a single instance in which the placenta was extruded in the manner he had stated, any hæmorrhage ensued.

DR. JOHNSTON thought that the Society should feel grateful to Dr. M'Clintock for bringing this subject forward, as *post-partum* hæmorrhage was one of the accidents which obstetric practitioners were liable to meet with. He was in accord with Drs. M'Clintock and Churchill in some matters, especially with regard to the propriety of giving ergot at the latter portion of the second stage of labour. In the Rotunda Hospital they always made it a point to administer ergot, especially in cases where there had been *post-partum* hæmorrhage on previous occasions, and with the expectation that they would be able to avert the danger to which they looked forward. As to rupture of the membranes in an early stage, he had not tried it. It might be egotistical on his part, but he should say that in the Rotunda they depended in a great measure on manipulation of the uterus. They made it a point to maintain a steady compression of the uterus from the time the head was being expelled, and never let go until the placenta was removed. Facts were stubborn things. He was about preparing a report of last year's cases in the Rotunda Hospital, and he would lay before them a portion of his statistics.

That a short second stage in labour, when the case is properly managed, has no relative tendency to induce *post-partum* hæmorrhage, would be seen by the following:—

*Cases delivered in the Rotunda Hospital during the year ending 5th Nov., 1873.*

In 26 cases the second stage lasted 5 minutes—all were pluriparæ. No hæmorrhage followed.

In 30 cases the second stage lasted 10 minutes—26 were pluriparæ, 4 were primiparæ. No hæmorrhage followed.

In 117 cases the second stage lasted 15 minutes—101 were pluriparæ; in 2 there was slight p. p. h.; 16 were primiparæ. No hæmorrhage followed.

In 37 cases the second stage lasted 20 minutes—33 were pluriparæ, no hæmorrhage followed; 4 were primiparæ, no hæmorrhage followed; and in 1 of these cases there had been severe p. p. h. on a former occasion.

In 209 cases the second stage lasted 30 minutes—177 were pluriparæ, no hæmorrhage followed; 32 were primiparæ, no hæmorrhage followed; 4 were twin cases—3 pluriparæ, 1 primipara, no hæmorrhage followed; 1 had p. p. h. on former occasions, none in this.

In 121 cases the second stage lasted 45 minutes—82 were pluriparæ, no hæmorrhage followed; 39 were primiparæ, 3 p. p. h. followed, slight; 1 case of twins, pluripara, no hæmorrhage.

In 146 cases the second stage lasted 60 minutes—98 were pluriparæ, 1 p. p. h., required solution of perchloride of iron; 48 were primiparæ, in one hæmorrhage followed, slight; 1, a case of twins, pluripara, no hæmorrhage followed.

Total, 686; 7 cases of p. p. h.—6 slight; 1 required the injection of solution of perchloride of iron; she recovered.

419 cases second stage were under half an hour, 337 being pluriparæ, in 2 slight p. p. h.; 56 were primiparæ, no hæmorrhage; p. p. h., 1 in 168½.

Four of the above were twin cases—3 pluriparæ, 1 primipara; no hæmorrhage. Two suffered from severe p. p. h. on former occasions, but none on this.

He attributed *post-partum* hæmorrhage, in a great measure, to want of proper manipulation. In former years it was the custom to put on a binder when the child was expelled, and to allow the placenta to remain from one to three hours in the uterus. Then the liability to *post-partum* hæmorrhage was very great, and some practitioners, through want of presence of mind, might not observe the rule laid down of putting the hand upon the fundus before the child was expelled. He saw a case lately where he was told the binder had been properly applied, and found the uterus above it, and he pressed out of it fully one pound of blood. Many of these cases, therefore, might be attributable to a want of proper manipulation, and sufficient pressure not having been kept upon the part. Certainly, so far as the statistics of the Rotunda Hospital went, there were not many cases of *post-partum* hæmorrhage. In the third stage of labour, out of 1,160 cases, 1,065 were completed in fifteen minutes. He impressed on the minds of the pupils never to hasten the third stage, but to keep up steady pressure. He would ask, why should they allow the placenta to remain a long time in the uterus? Surely, when they found

that by a little increase of steady pressure on the uterus they could remove the placenta, he could not conceive any reason why they should allow it to remain. Such was his experience, and that was the principle he endeavoured to inculcate on the gentlemen under his instruction, and he would try to impress it on his brethren in that room.

DR. CRONYN entirely agreed with what had fallen from Dr. Johnston; and, without disparaging ergot or early rupture of the membranes, both of which were valuable, he thought the proper use of the hand of the attendant was the most important matter, and was quite satisfied that, in most cases, it would be sufficient to prevent hæmorrhage. He had seen a great number of cases of hæmorrhage in hospital and out of it, and he came to the conclusion that if the duties of the medical attendant were properly performed towards the termination of the second stage of labour, in very few instances would hæmorrhage occur. He also agreed with the views expressed by the Chairman as to the importance of non-interference with the placenta. In some cases he had seen in private practice, he believed hæmorrhage had arisen from too hastily endeavouring to remove the placenta. A few minutes should be allowed to elapse after the birth of the child before attempting to press out the placenta. The uterus ought to be allowed a few minutes rest to recover itself—the too hasty pressing out of the placenta being calculated to create some irritation of the uterus, which, perhaps from causing an irregular action of the fibres or some other reason, resulted in hæmorrhage. He had had occasion to observe that some cases of hæmorrhage were caused by the attendants, who, in these instances, were merely nurse-tenders, not having removed the membranes properly. They hastily removed the placenta without taking away the membranes, which formed a source of irritation to the uterus. He might also observe as to the second stage that in many of the cases he had seen where the child and placenta were expelled with one pain, there was a total absence of hæmorrhage; but in those cases the patient had been properly attended to, and a judicious pressure of the uterus kept up.

DR. DARBY thought the discussion hitherto had not had much reference to Dr. McClinton's able paper—the object of which was to enable the obstetric practitioner, by certain diagnostic signs, to anticipate hæmorrhage in certain patients. The treatment of hæmorrhage when it occurred was, of course, a very important question, but it did not appear to him to bear much on Dr. McClinton's paper. It was a most difficult subject. He had met with patients who had had flooding to an enormous extent on one occasion, and no symptom of it in subsequent deliveries. He had met patients with the vacillating pulse, and no hæmorrhage occurred, but he had never seen a patient having a permanent quick pulse prior to delivery that some untoward symptom did not arise; probably



hæmorrhage, or some other occurrence that would induce any intelligent practitioner to be very much on his guard. So far as his experience enabled him to form an opinion, he thought it was most difficult to make up one's mind as to where hæmorrhage will or will not occur, unless they had the symptom of a quick pulse. He did not think that a patient who suffered much from hæmorrhage on one occasion was therefore liable to it on another. He had seen some marked cases of that kind, and had not found that hæmorrhage followed in subsequent confinements. With regard to the management of the case, every man must know the great importance of following down the uterus and keeping pressure on it; but with all this they would sometimes have hæmorrhage. No matter how watchful the attendant might be, the womb would occasionally slip from his grasp, and hæmorrhage would take place. As to the exhibition of ergot, he should be slow to give efficient doses of ergot before the placenta was expelled. He had seen, in consultation, one or two serious cases where the administration of ergot had the effect of causing irregular contraction of the uterus, giving rise to great difficulty in removing the placenta. In his own practice he never gave ergot until after the third stage. He preferred introducing his hand and removing the clots, and he believed very little, if any, bad consequences ensued, if that were properly done. He would much rather do that than give ergot before the placenta was expelled.

DR. DENHAM expressed his great admiration of the paper, and of the discussion which had ensued upon it. He believed, however, notwithstanding all that had been said that evening, it was a subject by no means exhausted. With respect to the pulse, he believed that in almost every case of labour, especially in the second stage, there would be rapid pulse, and he attached very little importance to rapidity of the pulse during that stage, as the excited nerves of the patient, the severity of the pains, and all the circumstances attendant on such cases, were calculated to cause that rapidity. But he attached the most marked importance to the condition of the pulse after the third stage had ceased, and he made it an invariable rule never to leave the patient's room if she had rapid pulse after delivery. In such a case he should go away with great hesitation and anxiety; but if the pulse fell to the normal standard, even though there should be a free discharge, he would leave with an easy mind. He believed the time to judge what effect the pulse would have on the future condition of the patient was after the binders were adjusted. In considering the subject of the expulsion of the placenta, they should go back to first principles. There was no immediate and direct connexion between the placenta and the wall of the uterus. They were only two bodies lying in apposition to one another, and it was quite possible for the placenta to be peeled off from the wall of the uterus

without a single fibre or blood vessel being ruptured. He believed hæmorrhage had occurred either from an irregular contraction of the uterus, or the anxiety of the practitioner to hasten the expulsion of the placenta by drawing it away, thereby causing a rupture of blood-vessels. He differed from some of his friends in using two methods for the removal of the placenta, viz., pressure on the fundus of the uterus and traction. Here, however, the *tactus eruditus* was necessary; but by combining the two forces—pressure on the one and traction on the other, and knowing to what extent he ought to do it—the practitioner could remove the placenta with quite as favourable results to the patient as if he left it to the exclusive powers of nature. They must, however, be guided by circumstances. There might be cases where it would be advisable to remain over the patient for half an hour or an hour, and there were other cases where the placenta would come away without any difficulty. Whether they liked it or not, sometimes the placenta would come away, and it must greatly depend on the experience of the practitioner, and upon his patience and judgment, to determine when he would take the placenta, not out of the uterus, but out of the vagina. As to ergot, he had devoted a great deal of attention to it. He believed the administration of ergot was very important, if given at the proper time, viz., towards the end of the second stage, when it would have a very marked influence in arresting hæmorrhage. If given after the expulsion of the placenta it was utterly powerless. He had related a vast number of cases where he gave ergot before labour set in at all, in cases where there was apparent abortion, and in other cases as an experiment, and he had never been able, by that means, to produce labour where there was none before. He had given it after the expulsion of the placenta, and had not seen a single instance where ergotic pains were produced when the drug was given after the placenta had been expelled. His belief was that, despite all precautions, cases of hæmorrhage would sometimes occur. He had attended patients who had a marked amount of flooding in their first labour. He had, on the other hand, seen a case where the labour went on for five days, and there was not a single drop of hæmorrhage. He had seen other cases where the labour went on naturally to the end of the third stage, and yet, after all, a gush of hæmorrhage came when it was least expected. He had attended those patients in their second confinement, and no hæmorrhage occurred. He had attended patients when their first, second, and third confinements were completed without hæmorrhage, and in the fourth confinement there was a gush of hæmorrhage. There were, therefore, cases where hæmorrhage would come on when they least expected, and could not anticipate it, and, consequently, they should always be prepared for such an occurrence.

DR. A. H. RINGLAND said, as regards the administration of ergot, he had a case the other night which might throw some light upon it. He was sent to see a woman. An experienced nurse—in whom, as a nurse, he could place perfect reliance—was with her. He had attended the woman in three confinements previously, and on each there had been *post-partum* hæmorrhage. He was sent for when the woman was advanced in the second stage, the head being on the perinæum, and, acting on what he had read, he administered a dose of ergot, giving her a drachm of “Long’s Extract.” The head was expelled very shortly afterwards. All went on well for ten minutes. He kept his hand on the fundus of the uterus, keeping up a firm pressure. The placenta did not come away; it was retained for forty minutes, and at the end of that time blood began to dribble, but seemed to have no effect on the woman, judging by the state of her pulse. He, therefore, introduced his hand, as a last resource, and removed the placenta. They knew morbid adhesions were very rare, and he thought a great many cases of hæmorrhage arose because there was not proper pressure on the fundus of the uterus.

THE VICE-PRESIDENT (DR. ATTHILL) said that in one of the papers recently published on the subject of *post-partum* hæmorrhage, Dr. Boulton, of London, advocated the application of the binder before the placenta was expelled, and Dr. Steele, of Liverpool, in a subsequent paper, while protesting against this practice, said he was aware that great stress was laid by the Dublin School of Midwifery on the application of the binder before the expulsion of the placenta. He (the Vice-President) wished to protest that such was not the practice of the Dublin School of Midwifery. They did not apply the binder until after the placenta was expelled, and he desired to take this opportunity of correcting what appeared to him to be an erroneous impression, existing among some, as to the practice in this respect adopted here.

DR. DENHAM said the practice in the Rotunda Hospital, in his early days, was to apply the binder before the placenta was expelled, but it was not the practice now, and he agreed with the Vice-President in condemning it.

DR. M’CLINTOCK, in reply, said he was much pleased at the indulgent criticism bestowed on his paper, especially as there was not a single original observation in it from first to last. He merely thought it would be desirable, at a time when the subject was under the notice of the profession, to bring it before the Society, and to allude particularly to two points, namely—the value of ergot as a preventive or a prophylactic of *post-partum* hæmorrhage; and, secondly, as to the utility of rupturing the membranes with the same object. That well-directed and careful manipulation of the uterus from the time of the expulsion of the child should contribute much to prevent hæmorrhage, he had no doubt; but

he took it for granted, in his paper, that the authorized mode of conducting the second stage was pursued. To say that hæmorrhage was always caused by overlooking that, would be untrue, for many of the most experienced practitioners had met with hæmorrhage, and it was unquestionable that, under the best manipulation of the uterus, hæmorrhage would occasionally arise, even to a very great and alarming extent. He attached the very highest importance to the practice of following down the fundus of the uterus, and it was a duty he always discharged himself. He agreed with the Chairman that the early or precipitate separation of the placenta was objectionable. Dr. Cronyn said well that the uterus should get a little rest and time to recover its strength. After the culminating effect it had made in expelling the child, he (Dr. M'Clintock) would not urge it to act for a few minutes, but would wait for a little time. Such was the established practice of the late Dr. Charles Johnson. He (Dr. M'Clintock) never was solicitous to expel the placenta for fifteen or twenty minutes after the child was born, unless hæmorrhage appeared. Levret seemed, in his remarks on this subject, to attach great importance to the point in question. With reference to the duration of the second point, he thought he had stated that the time for rupturing the membranes was when the os was fully dilated, or nearly so. If he had misrepresented Dr. Atthill in saying that he seemed disposed to give a preference to the use of the forceps over ergot (and such was his impression on reading Dr. Atthill's paper through), he had only to express his regret for the error. Dr. Darby had expressed great apprehension of the evil arising from giving ergot before the delivery of the child, viz., that in case the placenta happened to be morbidly adherent, and that the ergot caused strong contraction, it would increase the difficulty of removing the placenta. He (Dr. M'Clintock) did not dispute that position; but cases where there was a morbidly adherent placenta were very rare, and these cases of foreseen hæmorrhage were not very frequent; so that they would find the cases in which these two things were combined to be of extreme rarity. Dr. Denham's observations were important as to the administration of ergot before delivery. Where it was delayed until the hæmorrhage appeared, little benefit could be derived from it. Dr. Hardy was at pains to ascertain the time at which ergot began, after administration, to exercise its influence on the uterus, and the conclusion he came to was, that the specific effect of ergot commenced certainly not sooner than from 15 to 30 minutes after the dose was given; so that if it were not given until the hæmorrhage commenced, a fatal effusion of blood might take place before it could take effect. That showed the necessity of giving the ergot some little time before the expulsion of the placenta.

The Society then adjourned.



## PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. LYONS, President.

DR. BENNETT, Secretary.

*Medullary Cancer of Clavicle, with Secondary Dissemination.*—DR. BARTON said the case which he desired to bring under the notice of the Society was a contribution to the pathology of cancer. He would read briefly the notes which he had taken of the case. The patient was a young woman aged twenty-four, who was admitted to the Adelaide Hospital for the first time in last April. She was healthy from childhood until two years before the time of her admission to hospital, when she suffered from amenorrhœa. Six months later she noticed a small tumour, about the size of a pea, under the left clavicle, near the sternal end. This tumour greatly increased in size, and was the seat of pain of a darting kind. In six months' time it was as large as a hen's egg, and discoloured at the most prominent part, becoming of a dull purplish tint. When admitted to hospital in April last she was carefully examined, and the following group of symptoms was observed :—The tumour was of the size of a small orange, situated just beneath, and partly involving the sternal end of the clavicle. It was firm and fixed at the base, but most distinctly fluctuating at its most prominent part, the colour of which was a dark purple. The glands of the neck were enlarged, but she stated she had enlarged glands when a child. The mammary glands were free from disease. The pulse was about eighty, her appetite was good, her complexion rather florid. She had no pain, except an occasional lacerating pain through the tumour. As to the diagnosis, there seemed to be a possibility of doubt that the disease was malignant, and the diagnosis seemed to lie between medullary cancer and a deep abscess, connected probably with caries of the first rib. To fix the diagnosis an exploratory puncture was made into the tumour, from which nothing but a little blood was evacuated, consequently the opinion that it was a medullary cancer was confirmed. As nothing could be done for her, she left the hospital within the week. The most remarkable feature in the history of this case then followed, for shortly after she left he heard from a person that she was getting much better, and the poor woman herself thought she would recover. Dr. Barton was most anxious to see the girl, and a month after she left the hospital he saw and examined her, and found that the tumour had indeed diminished very much in size. A note taken at this time stated that the tumour had diminished to fully one-half the size it was when she left the hospital, but at the same time the

glands in the neck and in the axilla were more enlarged than at the time of her discharge, consequently there was no change in the opinion that had been formed of the case. She again claimed admission to hospital last October, having been absent for five months. The tumour was then the size of a cocoa-nut, and was of a dark purple colour. It had begun to grow again within three weeks before her second admission. It was very firmly fixed, and was much harder than on the former examination. The whole of the left side of the neck was occupied by large and hardened glands; the axilla was in the same state. The breathing was rapid and short. The posterior part of the left chest was found to be dull on percussion, and bronchial breathing was heard all over it. The right arm was found to be powerless. She could move the hand, but not the arm. The head of the humerus was found to be much enlarged, and participated in the disease.

The pulse was 114, there was very little appetite, and no sleep without hypnotics. The face was pink and white, and with none of that yellowish cachectic look characteristic of cancer. On the 10th of November her breathing was more difficult, and on Tuesday, the 11th, she died.

On making a *post-mortem* examination he found the tumour presented a firm aspect, and, both to the naked eye and under the microscope, it presented the characteristics of medullary cancer, but of a very firm texture. It had not penetrated through the rib, but it involved the sternal end of the clavicle, and dipped into the anterior mediastinum, which was filled up with this cancerous matter, and it extended to the base of the left lung, which was entirely carnified and infiltrated with cancerous material. The pleura was occupied with a blood-stained effusion, which seemed to have been the immediate cause of death; the bronchial glands were very much infiltrated with cancerous material, which extended into the substance of the lung. The other lung was also extensively diseased with cancerous deposits, about as large as the tip of the finger. The entire of the right humerus was removed, and it presented a remarkable example of secondary cancer in bone. The joint was perfectly healthy, but the bone immediately below the tuberosity was quite softened and brittle, and the shaft broke across when being removed from the body, illustrating the fracture which often takes place in cancerous patients during life. On a section he found the bone infiltrated with cancerous effusion, which entirely destroyed the cohesion of the osseous substance, causing that brittleness which it exhibited. This case suggested two questions in connexion with the formidable disease of cancer. In the first place, why did that malignant disease set itself up in this particular spot? There was nothing of a cancerous history in the case. One relative died of cancer of the breast, but beyond that there was no history of malignant disease in the father, mother, or immediate relatives. There was no

history of a hurt having been received in the place where the disease manifested itself. In the second place, why did secondary deposition take place in the humerus of the opposite side of the body? There was no apparent connexion between the shaft and head of the humerus and the cancerous deposition in the mediastinum. He could not undertake to answer the questions, but he thought the facts he had stated were worthy of being put upon record.—*November 29, 1873.*

*Cystic Disease of the Ovary.*—MR. PORTER exhibited an ovarian cyst which he had removed on the previous day (the 28th Nov.), from a patient in the Meath Hospital. She was an unmarried female, aged twenty-three years, and fifteen months previously she first noticed the development of the disease. It began on the right side. Before the operation she was tapped four times. The first tapping was performed on the 1st of December, 1872, when 190 ounces of fluid were taken away; the second in July, 1873, when 263 ounces were removed; the third on the 3rd September last, when 308 ounces; and the fourth in the last month when 204 ounces were drawn. It was an unilocular cyst, with a small amount of solid matter. There were two or three minute cysts attached, but a perfect absence of any appearance of malignancy about it. From the interior, for instance, there were none of those blue plumb-like growths usually seen in this disease when it was of a malignant form. It weighed 1lb. 15½ ounces, and the fluid contents were 210 ounces, the fluid being of a chocolate colour. When the tumour was distended the measurements were 26 inches in one circumference, and 29 in the other. The girth of the abdomen at the umbilicus was 36 inches, and from the pubis to the xiphoid cartilage it measured 14½ inches.—*November 29, 1873.*

*Fracture of Os Calcis.*—MR. PORTER exhibited the bones of a leg which he had amputated a fortnight ago. The patient was a boy, thirteen years of age. In endeavouring to jump on a tram-car, where the driver stood, his foot slipped, and he fell with his head towards the horses. One of the wheels of the vehicle passed along his left lower extremity. The muscles, when the boy was brought to hospital, were torn like ribbons, and there was very smart hæmorrhage. This was fortunately arrested at the time by Dr. Alcock, who happened to be in the tram-car, and who kept his finger on the femoral artery until the patient was conveyed to hospital. Mr. Porter then amputated the limb. The bones exhibited a fracture of the os calcis; the epiphysis at the lower end of the femur was separated, and a fracture passed between the condyles.—*November 29, 1873.*

*The Intestinal Lesions of Enteric Fever.*—DR. WILLIAM MOORE said the specimen which he now exhibited was illustrative of a disease with which

they should all be very familiar. It was a specimen of the pathology of a fatal case of enteric fever, but some points of the case possessed features of clinical interest. The patient was a young girl, twenty-one years of age, who was admitted to Sir Patrick Dun's Hospital on the 21st of October. On the night of her admission the temperature was  $105\frac{1}{2}$  degrees. There was no "spot" of any kind, and as far as could be ascertained the girl had then been eight days in fever. On the night of the thirteenth day of the fever, with a temperature of  $104$  degrees, very profuse hæmorrhage ensued. If he said it was between twenty and thirty ounces he would be under the mark. The result was that the temperature fell on the next morning, the fourteenth day of the fever, five degrees. It rose the next morning to the original  $104\frac{1}{2}$ . There was little or no hæmorrhage for a couple of days, but the temperature kept high. On the nineteenth day it fell to  $101^{\circ}$  in the evening, and on the night of the twentieth day there was another attack of hæmorrhage, not so profuse as the first, and the temperature fell on that occasion three degrees. The temperature again rose to  $104^{\circ}$ , and then she had pneumonic signs. The day before her death the morning temperature was  $105^{\circ}$ , and the evening temperature  $106^{\circ}$ , and a few hours before death the temperature rose to  $108^{\circ}$ . From first to last there was not a single spot of any kind indicative of enteric fever. The intestine presented a good specimen of the ulceration of enteric fever. They would see the ulceration beginning at the top of the ileum. The ulcerations were large, and the base of some of them was formed by mere peritoneum. As they approached the cæcum the ulcerations became larger; some of them being irregular in shape and from one and a-half to two inches in size, and close by the cæcum there was a perforated spot. This perforation, however, did not occur during life, but the moment the gut was handled the piece fell out. Two small circular ulcers were found in the cæcum. He brought forward this case because of some points of special clinical and pathological interest. The fact of the high temperature of  $105\cdot5^{\circ}$  on the eighth day was of interest as regarded the prognosis of the case. Again, he thought the fall of *five degrees* in the temperature after the occurrence of hæmorrhage—and all these variations of temperature were most carefully observed by Mr. Forsythe, the resident pupil—was an interesting clinical fact. The fall in the temperature bore a close relation to the quantity of the hæmorrhage. On the second occasion, when there was a smaller hæmorrhage, the fall was relatively less, being only *three degrees*. Again, this case showed that they might have a most aggravated form of enteric fever without the presence of any eruption, and that therefore there was no direct relation between the presence of eruption and the severity of the disease. Here there was not a single lenticular spot from the beginning to the end, and yet the case was, all through, a most severe one.—

*November 29, 1873.*



*Osteitis*.—DR. BARTON showed a leg which he had amputated on the previous Wednesday morning. The woman came from the West of Ireland, having been suffering for a year or even more from disease of the left knee-joint. Her history was as follows:—She was married and had several children. After each confinement she suffered from glandular abscesses, either in the groin or in the muscular tissue of the leg. These abscesses had healed up, leaving their strumous-looking cicatrices. After her last confinement, a year and a-half ago, the left knee was the seat of inflammation, and instead of subsiding or ending in a simple abscess in the soft parts, the joint became affected. After some time the disease progressed towards the entire destruction of the joint, and she undertook the journey to town for relief. When she came under his observation he found her greatly emaciated, the pulse 100, very pale and anæmic, and with marks of abscesses in both groins and also in the right leg, one of which was still open. The left knee was flexed upon the thigh, and turned on the outside. The joint was globular in shape, measuring  $2\frac{1}{2}$  inches more in circumference than the other, extremely tender on touching it, with severe starting pains at night. There were two sinuses behind in the popliteal space, and anteriorly on the inner part near the head of the tibia. These led into cavities that seemed to extend through the cellular tissue. The poor woman strongly objected to amputation, but as time elapsed her system became more weakened, diarrhœa set in, there was marked hectic, and the knee became still more tender, and the seat of still greater pain. She therefore agreed to permit amputation, being informed that it was the only thing that could be done for her; and the limb was, accordingly, amputated on Wednesday morning. The specimen presented an excellent example of osteitis, and of its progress from the expanded extremities of the bones into the knee-joint. A section made of the head of the tibia showed the centre part to be perfectly softened and broken down, the cancelli of the bone being infiltrated with reddish matter, and in the middle with a yellowish material. This was, doubtless, the commencement from which the inflammation opened to this joint, removing the cartilage, in place of which the head of the tibia was covered by a pultaceous matter, an exudation of a lymph material. The same destructive process had gone on round the condyles of the femur, and at the patella there was ulceration of the cartilage, with a pink colour of the bone, which was soft and readily cut with a knife. The cancelli of the bone were the original seat of the disease, from which it extended into the joint, involving the cartilages and the synovial membrane and so destroying the articulation. The specimen before the Society illustrated remarkably well the destructive progress of inflammation of bone, when in a weakly strumous person it becomes located in the expanded extremities, extending thence into the neighbouring articulation.—*December 6, 1873.*

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

### CONTENTS.

THIRD SERIES, No. XXVI.—FEBRUARY 1, 1874.

#### PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. IV.—Observations on Excision of the Knee-joint, and its After-treatment by means of Dr. P. H. Watson's Plaster-Splint. By HENRY J. TYRRELL, F.R.C.S.I., M.R.I.A.; one of the Surgeons to the Mater Misericordiæ Hospital, &c.—(Illustrated), - -	97
ART. V.—Rest; a Therapeutic Agent in the Treatment of Disease, more particularly of Disease of the Circulatory System. By J. MAGEE FINNY, M.D., Univ. Dubl.; Fellow of the King and Queen's College of Physicians in Ireland; Demonstrator of Anatomy, School of Physic, T.C.D., - - - - -	108

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. A Practical Treatise on the Diseases of the Heart and Great Vessels, including the Principles of their Physical Diagnosis. By WALTER HAYLE WALSH, M.D., Emeritus Professor of the Principles and Practice of Medicine in University College, London, - -	150
2. Works on Diseases of the Skin:—	
I.—Skin Diseases: an Inquiry into their Parasitic Origin and connexion with Eye Affections; also the Fungoid, or Germ Theory of Cholera. By JABEZ HOGG, - -	134
II.—Elephantiasis Græcorum, or True Leprosy. By ROBERT LIVEING, A.M., M.D., Cantab., - - -	135
III.—Photographic Clinique of the British Hospital for Diseases of the Skin, - - - - -	135

## PART III.—HALF-YEARLY REPORT.

Report on Materia Medica and Therapeutics. By WALTER G. SMITH, M.D., <i>Dubl.</i> ; Fellow and Censor, K. & Q.C.P.I.; Examiner in Materia Medica, Q.U.I., - - - - -	136
---	-----

## PART IV.—MEDICAL MISCELLANY.

## Transactions of the Medical Society of the College of Physicians:—

Climate and Vital Statistics of Tasmania. By J. W. MOORE, M.D., <i>Dubl.</i> ; <i>Dipl. State Med.</i> , <i>Trin Coll.</i> , <i>Dubl.</i> ; F.K.Q.C.P.I., -	151
On the Use of Bromide of Potassium in the Treatment of Epilepsy. By THOMAS HAYDEN, F.K.Q.C.P.I.; Physician to the Mater <i>Misericordiæ Hospital</i> , - - - - -	163

## Proceedings of the Dublin Obstetrical Society:—

Uterine Tumours. By LOMBE ATTHILL, M.D., F.K. & Q.C.P., Vice-President, - - - - -	174
A Report of the Rotunda Lying-in Hospital for the year 1873. By GEORGE JOHNSTON, M.D., F.K. & Q.C.P., Master of the Hospital,	177

## Proceedings of the Pathological Society of Dublin:—

DR. W. MOORE on the Intestinal Lesions of Enteric Fever (omitted in last Number), - - - - -	94
DR. BENNETT on Fatal Incised Wound of the Abdomen, - -	205
DR. T. E. LITTLE on Comminuted Fracture of the Patella, - -	207

Books Received, - - - - -	- Cover.
---------------------------	----------

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

FEBRUARY 1, 1874.

---

### PART I.

### ORIGINAL COMMUNICATIONS.

---

ART. IV.—*Observations on Excision of the Knee-Joint, and its After-treatment by means of Dr. P. H. Watson's Plaster-Splint.*  
By HENRY J. TYRRELL, F.R.C.S.I., M.R.I.A.; one of the Surgeons to the Mater Misericordiæ Hospital, &c.

MY object in the present communication is not to discuss the propriety of excising the knee-joint, or to contrast the operation with amputation through the lower third of the thigh, for I agree with, I believe, nearly all experienced practical surgeons, that the operation, although a more severe one than amputation, more dangerous to life, and requiring a longer time for convalescence, should be practised in certain cases, because its results are so brilliant when it succeeds that we are warranted in running some additional risks to secure them.

Having had occasion to excise the knee-joint twice last year, I used, with both patients, Dr. Patrick H. Watson's, of Edinburgh, splint. In the present paper I propose to bring it under the notice of the profession here, and to strongly recommend it as the most convenient, the safest, and by far the most comfortable splint for the patient, and also as the one requiring least after-care on the part of the surgeon.

Although Dr. Watson published his memoir on Excision of the Knee-joint in 1868—and although Holmes, in his work on the Surgical Diseases of Children, and other authors, speak most



highly of Dr. Watson's method of treatment—as far as I know, no surgeon in Dublin has hitherto given it a trial except my colleague, Mr. Hayes, who used it, last June, in a case, published by him in the January number of the *Irish Hospital Gazette*.

Dr. Watson<sup>a</sup> gives the following description of the apparatus:—It consists essentially of two parts—1, a suspension rod made of iron, about the size of No. 5 of trade wire gauge; 2, a modelled Gooch (scored) splint, long enough to extend from the tuberosity of the ischium to beyond the heel.

The suspension rod extends from the groin to the extremities of the toes, and is bent to the outline of the limb, departing from it only in the situation of the excision, where it forms a bow or arch. To the upper surface of the rod are attached one or more hooks, by which suspension is effected.



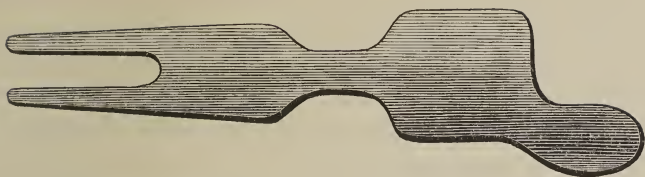
Suspension rod for front of limb. The arch corresponds to the site of the excision. The suspension hook to the ankle-joint. Upper end should terminate at the fold of the groin.

The Gooch splint should not be made too wide, and should certainly not surround the thigh and leg more than two-thirds of their circumference. It should be scooped away laterally, at the point corresponding to the tendo-Achillis and heel. The inferior extremity of the splint is thus of a horse-shoe or stirrup shape, and admits of the ankle and foot being supported by the lateral horns of the splint, as they fold on each side of the malleoli, without the risk of inordinate pressure being made upon the tuberosity of the os calcis.



Posterior "Gooch" splint of ordinary form, hollowed out in each side, on the level of the operation wound, and cut out at the lower part in a horse-shoe or stirrup form, so as to relieve the heel and tendo-Achillis from pressure.

<sup>a</sup> Excision of the Knee-joint; a Description of a New Apparatus for the After-treatment, with Illustrative Cases. By Patrick H. Watson, M.D., F.R.S.E., F.R.C.S.I., &c. Edinburgh: MacLachlan and Stewart. London: Robert Hardwicke.



Alternative form of "Gooch" splint for excision, hollowed out as in Fig. 2. The rounded part above adapted to the os innominatum, and secured by means of strips of adhesive plaster. (*This latter splint I have not used.*)

In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffin, in the situation which corresponds to the site of the operation. The iron rod is then placed in front, and folded lint laid between it and the limb at the groin (where the rod terminates above) at the upper part of the tibia, and at the bend of the ankle. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffin, which, having been rendered temporarily liquid by heat, is applied by a large paint-brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof bar of the common iron wire cradle, employed to support the weight of the bed clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffin is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffin, but it has the disadvantage of permitting soakage of discharge, it takes longer to consolidate, and when consolidated is less easily chipped through, by means of a bandage-shears, than the paraffin apparatus. I have, of late, in the treatment of excisions and of compound fractures, employed the plaster-of-Paris as the substantial substratum, and thereafter applied the paraffin over it, as soon as consolidation and drying have become complete, so as to secure its greater toughness, and its complete impermeability to fluids of any kind—whether blood, or serum, or pus—from the line of incision, or water from the wet dressings which may be applied during the healing of the wound. The foot and limb should not be much raised above the level of the mattress upon which the patient lies, as this is apt to lead to an awkward position of the limb when the patient first begins to

move about. When in bed, the sacrum and hips must be protected from all risks of inordinate pressure by means of a large square corrugated air-cushion, completely covered with a blanket or draw-sheet. This will be found to be most evenly and comfortably supported upon a firm hair mattress, laid either on a thick hair palliasse, or, better, a spring bed. The patient should be encouraged to sit up as much in bed as he can, even within a day or two of the operation; and, as soon as it is possible, should be removed out of the bed during the day, either to another bed or couch, or, should such not be attainable, to a mattress laid on the floor. In sitting up, the air pillow, placed beneath upon the seat, will be found a great comfort, as it admits of easy relief to the irksomeness of maintaining nearly the same sitting posture for a period of several hours.

CASE I.—Mary D., aged fourteen years, was admitted into the Mater Misericordiæ Hospital on the 1st of March, 1873, for disease of the right knee-joint.

For the last two years she had been in various hospitals, at different times; and, her friends not being willing to allow her leg to be amputated, she left, without deriving any benefit from treatment.

On admission the knee was found to be much enlarged, white in colour, and when at rest she did not complain of much pain; but when the heel was percussed, or the leg rotated, she complained bitterly. There were three sinuses discharging pus—one opposite the external condyle, two corresponding to the inner side of the head of the tibia. A probe passed through the openings entered the joint, and gave the sensation of grating against roughened bone. On rotation of the leg, and placing the hand over the knee, it became apparent that the cartilages were gone, and the ends of the bones bare, irregular, and rough. The leg hung like the handle of a flail, and was much atrophied. The little girl was very thin and exhausted from long confinement, but there was no evidence of any visceral disease. She stated that three years before she fell and hurt her knee, and that since she has been a cripple.

She was ordered to remain in bed, a splint was applied to the leg, and every attention paid to her general health; and as it was evident an operation would be required, I ordered her 15 m of the tincture of perchloride of iron three times daily (a practice I invariably adopt when time permits before performing any serious

operation, as I believe, to say the least of it, it tends to lessen very much the liability to erysipelas and pyæmia).

Looking on the case as one of primary disease of the synovial membrane, believing the bones to be but little affected, and the girl's health tolerably good (also she was most unwilling to lose her leg, and begged of me not to cut it off), I determined to excise it.

Knowing how difficult it has proved, in all the cases of excision of the knee that I have seen treated in Dublin, to keep the parts in position with the usual box splint—how irksome and constrained the patient's position must of necessity be—how unfavourable such a position is to a speedy convalescence—how it must necessarily render the operation more dangerous to life—I determined, if possible, to avoid using it; and having had considerable practice in the use of plaster-of-Paris in the treatment of simple and compound fractures and diseased joints, I was struck with the simplicity of Dr. Watson's apparatus, and made up my mind to give it a trial. Before doing so, however, I tested it in the following manner:—I gave Dr. Watson's pamphlet to Mr. Corcoran, of the firm of M'Adam and Corcoran, instrument makers, and desired him to measure my patient's leg and thigh, and to make the scored splint and suspension rod according to his directions. Having procured the splints and rod, I applied them to the diseased leg, and allowed the apparatus to remain on for two days. It proved, so far, a decided success; my patient expressed herself pleased with it; it gave no pain, allowed her, when it was suspended by a piece of bandage from an ordinary bed-cradle, to turn and shift her position with the greatest ease and comfort.

As the suspension rod moved a little from side to side, I directed the instrument-maker to make another for me, adding two small flanges of tin, each about two inches in diameter, which (I hope to be pardoned for saying) is, I think, an improvement, as it entirely prevents *wabbling*.



The flanges, being thin, are easily bent to correspond with the convexity of the leg and thigh.

On the 26th of March, 1873, I operated in the following manner:—



The patient having been put fully under the influence of ether, a tourniquet applied, and an assistant having taken charge of the sound leg, I entered the point of a strong scalpel above and behind the internal condyle of the femur, pushed it down to the bone, and then carried the incision downwards in a curved direction, severing the ligamentum patellæ, and then upwards and outwards to the upper and back part of the external condyle, where it terminated exactly opposite to where it commenced. I then dissected up the flap with the patella contained in it. I then removed the patella, and next, having divided the lateral ligaments, an assistant having forcibly bent the leg, I carefully cut across the crucial ligaments, and caused the condyles of the femur to project from the wound. I next cleared the condyloid surface of the femur for the saw, *as I well knew if I removed the whole epiphyseal line arrest of growth should follow*. I carefully avoided doing so by making a horizontal section from before backwards, commencing in front *below* the upper margin of the trochlear surface of the femur. I then removed a thin slice of bone from the tibia, and having clipped away with a strong curved scissors all the thickened synovial membrane, I twisted four vessels, sponged out the wound with a solution of chloride of zinc (10 grs. to the oz.), and finally, having inserted two *pledgets of tenax* (rolled into plugs about four inches long, and as thick as the little finger) into the most depending angles of the wound, the limb was straitened, and I commenced to apply the apparatus in the following manner:—I first carefully wiped away all blood from the thigh and leg, and thoroughly dried the surface. I next applied a thin flannel roller from the toes to the groin (except for six inches at the knee), and then adjusted the limb on the posterior splint, which was previously well padded with fine cotton wadding, particularly in the centre corresponding to the popliteal space, and opposite the two malleoli. A piece of gutta-percha tissue was laid on over the cotton wadding in the popliteal space. The iron suspension rod was next applied in front, and a little wadding placed here and there under it made it lie accurately. A number of coarse muslin bandages, each three inches broad and six yards long, having been previously prepared by rubbing plaster-of-Paris into them, and rolling them up dry, were immersed in water, and quickly applied from the toes to the bow of the iron rod, and then under the knee and up the thigh to the groin. The bandages were applied three-fold, and a little fluid plaster afterwards, laid on with the hand, strengthened and at

the same time rendered the whole apparatus more artistic looking. The edges of the wound were united everywhere but at the angles (here, as before mentioned, two plugs, or, more properly, two most efficient drainage tubes, were inserted) by four carbolized gut sutures. The splint in the neighbourhood of the knee was then well painted with fluid paraffin, and a large pledget of tenax *covered* with muslin was placed on the wound. The ends of the lateral drainage tubes were placed in the tenax over the wound, and a bandage, pretty tightly applied round the knee, under the iron bow, kept all secure. The patient was then removed to her bed, previously prepared by placing a firm hair mattress on a spring-bed, and the limb was suspended by a piece of bandage passed from the lower hook to the roof of a wire bed cradle—the heel being raised one inch from the bed. When she fully recovered from the anæsthetic effects of the ether, she became very restless—in fact, she became quite drunk and difficult to restrain. She sat up in bed, and wanted to get up altogether. Towards evening she became more tranquil, took some strong beef-tea, and a glass of wine, and at ten o'clock p.m. she had a draught containing 15 m of Battley's sedative and 15 grs. of chloral hydrate. Immediately after taking the draught she fell into a tranquil sleep, and did not awake till six o'clock the following morning.

March 27th.—Is a little hysterical, but otherwise perfectly comfortable; not much pain in the wound; pulse 95. Ordered to take beef-tea, milk, and arrowroot biscuits, and 4 oz. of wine; the anodyne to be repeated if required.

March 28th.—Did not require the anodyne; pulse 92; slept well.

March 29th.—Slept well; took her breakfast sitting up; settled her hair herself; says she is quite comfortable.

March 30th.—Passed a good night; pulse 88; removed the dressing, and drew out the drainage tubes. The dressing was perfectly saturated with a sanguineo-serous discharge. Except where the drainage tubes were inserted the wound is quite healed.

Before applying the tenax dressing again I softened a piece of paraffin with my fingers sufficiently to permit me to insert it like a wedge between the skin and the plaster bandage at the angles of the wound. This efficiently prevented any discharge from trickling into the popliteal space.

April 10th.—Every second day the tenax dressing was renewed since last report, and before re-applying it the whole exposed

surface of the knee was sprayed with a weak solution of carbolic acid (10 grs. to the oz.). There is a moderate discharge of pus from the inner angle; the outer angle almost healed. The gut sutures have come away in the usual way by absorption of the portions inserted into the living tissues. She sits up every day, moves about from one side of the bed to the other. Several surgeons who have seen the case are quite pleased with the apparatus, and Dr. Ladly, from the United States, remarked that the only part of her person she could not injure by falling out of bed was her resected knee.

The wood-engravings are taken from photographs executed on November 20, and they speak for themselves.



May 21st.—Removed the splint to-day for the first time; union perfect; slight mobility; operation wound long since healed. The

openings of two of the sinuses present before the operation not quite healed. Re-applied the apparatus more lightly; cut off the portion of the splint projecting beyond the foot; allowed to move about with crutches.

June 14th.—Walks with one crutch; spends most of her time in the pleasure-grounds of the hospital.

Nov. 20th.—Splints all removed; perfect bony union; she walks and runs with perfect ease. A cork heel in the inside of her boot,  $\frac{3}{4}$ -inch thick, prevents limping, and as she walks or runs no one would suppose she had been subjected to so severe an operation.



The wood-engravings, natural size of the portions of the bones removed, show accurately the amount of disease present, and the peculiar worm-eaten appearance of the articulating surfaces of the femur and tibia. The cartilage was also removed from the patella, but otherwise it was not diseased.



CASE II.—Thomas C., aged ten years, a deaf mute from St. Vincent's Asylum for the Deaf and Dumb. was admitted into the Mater Misericordiæ Hospital on the 10th of March, 1873, under the care of Mr. Tyrrell.

On admission we were informed that the boy had been labouring under disease of the knee-joint for two years. The affection did not arise from accident. The disease was at no time attended with much pain, and until six months ago he was able to get about with a crutch. Since then he preferred remaining in bed. The medical officer of the institution had employed various remedies, but without benefit.

On examination the joint was found to be very much swollen, globular, elastic to the touch, painful when pressed on, and at three points fluctuation could be felt. The boy was small for his age; had been always delicate and dull; his head was large and misshapen. He had a slight cough, but there was no evidence of organic disease of the lungs, kidneys, or any of the other viscera. The case was evidently one of pulpy degeneration of the synovial membrane in an advanced stage, and I recommended amputation; this, however, the boy's father would not hear of.

Under these circumstances I gave a trial to rest and Scott's bandage locally, and the administration of cod-liver-oil and iron internally. This treatment was continued for a month without benefit. Three abscesses formed on the knee and burst externally, and on examination with a probe were found to communicate with the joint. Amputation was again proposed, but would not be permitted. Although I looked upon the case as one unsuitable for excision, owing to the enormous thickness and diseased state of the synovial membrane and the general unhealthy state of the boy, I gave him the chance, as I had no other alternative.

On the 25th of April I operated, as in the first case. In reflecting the soft parts to expose the bones, a quantity of thick, gelatinous fluid escaped. The synovial membrane was fully one and a half inch in thickness in some parts. It was of a yellowish brown colour, soft, and easily broken down. The cartilages were softened and easily scraped off the bones. The bones were a little expanded, but otherwise not perceptibly diseased. The patella was literally buried in the synovial membrane. I removed, with an ordinary saw, the extremities of the femur and tibia, as before described, and also dissected out the patella, and then, with a strong curved scissors, clipped away as much of the diseased soft parts as I could; much,

however, was left. The wound was sponged with a solution of chloride of zinc. No vessels required to be twisted or ligatured. The immovable apparatus was applied, as in the first case, and the patient removed to bed, and the leg swung to a cradle. As soon as the little boy recovered perfectly from the ether, fifteen drops of Battley's solution were given.

April 26th.—Passed a good night; seems quite cheerful, and moves about in his bed with ease.

June 14th.—Everything going on well; the same dressing was used as in the other case. He hooks his leg up and lays it down on the bed himself when he is desired to do so by signs. The operation wound nearly healed; the sinuses show no disposition to heal.

July 18th.—Removed the splint for the first time. No union; position perfect. Re-applied apparatus.

August 10th.—For the last few days he has declined much in health. Appetite failing; he sweats at night; pulse 110; no cough; urine albuminous, specific gravity 1.010. He was removed to another ward, and shifted every day from his bed to a sofa placed in the open air.

August 17th.—Complains very much of his head, the light gives him annoyance, and he excludes it by burying his forehead in his pillow, and drawing the sheet over his head. The poor little dumb fellow excites great compassion from his inability to make known his wants. On the 20th he had a fit; his sphincters became relaxed; he lay in a semi-comatose state for three days, and then died.

The knee was the only part we were allowed to examine. The ends of the bones were in good position, but not the faintest attempt at union had taken place.

Although the result of this case was unsatisfactory, still, for my present purpose (*viz.*, to prove how efficient and comfortable the immovable apparatus of Dr. Watson is), it answers, perhaps, even better than a successful one.

The case was an unpromising one from the beginning. The operation was not one I would have selected had I liberty to choose. Still, from the day the operation was performed till the patient died (three months), the leg gave him no trouble; when suspended from the cradle he could move freely from one side of the bed to the other, he could hook it up and lay it down himself without trouble or pain. There was no tenderness at any time since the operation over the sacrum or nates; and I believe I can, with

truth, assert that with any other splint he would not have lived so long—nay, more, I do not consider the operation to have hastened his death; but, on the contrary, to have retarded it; inasmuch as he suffered less inconvenience, was less constrained in his movements, more master of his own actions, was in every way better able to assist himself after the operation than for months before. He died, not from the operation, but in spite of it.

Before I conclude this paper I shall refer to the use of tenax as a dressing, and to my mode of using it. When I introduced it first, about eighteen months ago, in the Mater Misericordiæ Hospital, it was found occasionally to give rise to extreme irritation when applied to a granulating surface, or to the skin of a delicate female (as after amputation of the breast); to obviate this I have, for the last twelve months, first placed over the wound or surface to be dressed *a piece of thin washed muslin*, and then the tenax. This mode of dressing answers admirably, and is in constant use in the hospital. It prevents the fibres of the tenax from irritating; and when you wish to change the dressing you do so with great ease and cleanliness by raising the muslin, and with it the superimposed tenax saturated with the discharge. Ordinary lint is the very worst application that can be applied to a sore or suppurating wound, its want of capillarity prevents any discharge that may be present from flowing away. After amputations, resections, and, indeed, all operations, I invariably use the tenax in the manner described, and, as a rule, do not remove the first dressings for four days.

ART. V.—*Rest: a Therapeutic Agent in the Treatment of Disease, more particularly of Disease of the Circulatory System.*<sup>a</sup> By J. MAGEE FINNY, M.D. Univ. Dubl.; Fellow of the King and Queen's College of Physicians in Ireland; Demonstrator of Anatomy, School of Physic, T.C.D.

IN the present day, when the medical mind is, so to speak, more particularly directed to the inquiry as to the medicines which are useful, and the drugs which, from their uncertain preparation, and their still more uncertain effects on the diseased conditions of the human body, are to be labelled “useless”—when some of the first men of the day preach to their classes, and publish in the

<sup>a</sup> Thesis for the degree of M.D., conferred by Trinity College (*stip. cond.*) at the Winter Commencements, December 17th, 1873.

periodicals, the pernicious doctrine of universal empiricism, and others support the purely expectant system, waiting for signs, which, perchance, may not be given them—at a time when the minds of thinking men are awakening out of the lethargy of scepticism in the use of medicine which quite recently raged with all the virulence of the inroad of an epidemic, threatening even to overwhelm our faith in the science of therapeutics—when the working men are shaking themselves loose from the idle crowd of gossiping theorists, and, with Drs. Richardson, Harley, Frazer, and others, as their pioneers, are making the way clear to an accurate acquaintance with drugs, their physiological action upon healthy man, as well as upon the lower creation, the requisite dose, and the best mode of administration—at such a time it may not be altogether out of season to consider the valuable therapeutic agent we have at our very hand in Rest, physical or mechanical, and its equally great, equally powerful handmaid, physiological rest.

It seems to me that too much stress has hitherto been laid, and, as one may judge from the perusal of the numerous periodicals of the day, still continues to be laid, upon the supposed efficacy of such and such a medicine, given in such and such a dose, and that too little stress is laid upon, too little attention paid to, the many and valuable means, natural and artificial, which are at the constant disposal of the physician.

The cause for thus *erroneously limiting* therapeutics to the employment of medicines and their effects, is hard, if not impossible, to arrive at; but, it appears to me, it may arise from a faulty and narrow view taken by both the physician and his patient. The *post hoc, ergo propter hoc* maxim is so manifestly that on which the public delight to pin their faith—and as, with more zeal than wisdom or prudence, they applaud or deprecate such and such a drug, such and such a nostrum, proportionately as in some particular case the medicine or the nostrum was followed by either a favourable or unfavourable result—that it is sometimes hard for the physician to avoid accepting the self-satisfying oblation of cleverness displayed in the selection and employment of the medicine, which is often too readily tendered by the patient or the patient's friends; and still harder is it to believe the medicine, or mode of employing it, had little or nothing to say to the direct cure of the patient, and for the practitioner to acknowledge and freely confess to himself, after strict and honest examination, that the result was due to a train of physiological processes, very often



most complex, which he had failed by his medication either to guide or to interrupt. Ignorance, and its ill-favoured consort, Vanity, are, I believe, too often, the *fons et origo mali*. It is so tempting to the physician to give his prescription the credit of the result, and to congratulate himself on his discriminating skill, and his patient on his no less deserving discrimination in the selection of so successful a medical attendant. This ignorance is pardonable in not a few instances, and the fault lies, *not at the feet* of the medical attendant, but of the system of medical education and medical examination by our governing boards. Therapeutics, as a science, is neglected; *Materia Medica* and the *Pharmacopœia*, with its multifarious drugs, alike *useful and useless*, are taught; and thus the student goes forth with a heavy mail of armour, with weapons for his right hand and his left, but how and when to use them, whether for defence or offence, he knows not.

As it is not my intention or desire to enter into the remedies for these abuses—which, by the way, might prove a task of more than Herculean difficulty to remedy in this hour of competition and this day of hurry—I shall consider Rest, physical and physiological, as a curative means of immense importance, and, in the light of its being a guide to treatment, shall impress the advantage to be gained by a due appreciation of this agent in the treatment of disease.

The principle is not a new one, nor its application novel. It has been recognised and valued at all times by the thinking and successful practitioner, and, no doubt, has constantly been acted upon, perhaps for the most part, blindly, by those who follow a routine line of practice; but it is not, I am convinced, as clearly or as often before the eye of the physician as I deem it ought to be, nor is it sufficiently inculcated in the writings and published works of the best authors, who deal with the treatment of disease.

As an agent of great value in the treatment of surgery, it is, perhaps, more generally recognised and adopted than it is in medicine—as, for example, in external wounds, fractures, or displacements of bones, inflammation of joints, &c., &c. It was on this principle, and with this object, that Pott suggested the treatment of all fractures of the bones of the extremities (with the exception of the patella and olecranon) should be by rest in the flexed position; or, in other words, by muscular, that is physiological rest. Rest must be obtained to secure the union of bone, or solution of continuity in the superficial parts; and, should the surgeon fail to

assist nature, nature herself may employ means both to ease the pain which motion, or unrest of the parts gives, as a warning and a demand for rest, and also to knit the disunited parts together again. This protective care of nature we see in the vital ankylosis of the muscles of the thigh in morbus coxæ, in a similar state of the rectus abdominis in hepatic abscess, and fixation of the side in pleurisy.

Dr. James Macartney, Professor of Anatomy in the School of Physic, Trinity College, thus expresses this doctrine in his excellent Treatise on Inflammation (1838. Page 198):—

“It is obvious that every *movement* to which a wounded or inflamed part is subjected, must act upon the one like a repetition of the original injury, and upon the other like a continuance of the irritating cause. Rest is not only a negative advantage, as saving the patient from renewed injury or irritation, but a positive remedy, as it diminishes the heat of the body, reduces the pulse, and alleviates pain.” . . . “Rest is of so much value in the treatment of inflammation, that in some instances no means will advance the cure without it, and numerous injuries would do well with perfect rest and nothing else.”

In considering this therapeutic, it becomes necessary to clearly understand what is meant by rest.

To the exhausted frame of a healthy man rest is necessary, and sleep to the weary brain. Two ends are thus accomplished; first, by rest, further waste is checked; and, secondly, nature, during the hours of repose and sleep, repairs the waste produced during the hours of activity. Rest and sleep do not, of themselves, restore the power to the weary limbs or vigour to the flagging spirits; they do but place the individual in the best possible position for nature's recuperative powers to exercise their sway without detraction or interruption. In different individuals—or in the same individual at different periods of his life, and under differing external circumstances—this vital power varies; and, accordingly, we find, in direct relation thereto, the duration and depth of sleep will vary. But under other forms and under other aspects is fatigue met, and energy re-infused; the light story, the entertaining narrative, the strains of music, the change of scene and society, are every-day roads by which rest is brought to the taxed brain of the student or merchant; nay, even at times there is relaxation found in great physical exertion, and while the body is undergoing fatigue and performing unwonted feats, the mind may be at rest, and thus regain fresh life

and vigour. The summer tour to the higher altitudes of the Swiss alps, to the over-worked lawyer or medical man, is a familiar but true instance of this fact. How invigorated his mind, how free his thoughts, how enduring his mental powers, as he returns to town! and yet, for weeks, perhaps, he may have walked further, slept less, and lived more lightly than has ever been his wont, or was ever in his power to accomplish while at home.

Rest is not only an agent of repair, but prophylactic in its nature; and, as such, is constantly acted on by the public at large and well recognised by them as an integral part of success in life, be it in a man's own frame, or in that of his subordinates, or of his beasts of burden; while on the other hand, by the neglect of this agent, it is more than probable that many a useful life is lost, or irreparable mischief inflicted upon the brain and spinal marrow, and many a victim is yearly sacrificed at the shrine of fevered ambition in the restless service of Mammon.

As the rest necessary for the preservation of health of mind and body, in every-day wear and tear of life, is thus diversified in its nature, and in the mode of organizing it, so must it be when applied as an adjunct to the treatment of disease. The one goal may be reached by many roads, in not a few instances, by seemingly opposite routes—in one case by depletion, in another by stimulation—in this by exercise, in that by bodily repose. In a word, as rest is nature's means of repair, in health and disease alike, it may be defined to be the best possible condition in which a diseased organ can be placed, so as to allow the disturbed equilibrium between supply and demand to be restored. It is in this view it occurs that, while a physician does not cure a disease—be it a fever or a local inflammation—by this medicine or that, his object is, nevertheless, the cure of his patient—his means, such rules as to diet, sleep, and exercise, such medicines, as will mechanically or physiologically best tend to regulate the forces, and thus allow Nature free play in restoring the patient. All through life, two tendencies are at work, as in the moral so in the animal world, for good and for evil, for life and for death; the physician's duty is to see that he counteracts the baneful tendency and helps the failing powers. In no case is this more evident than in that of essential fever. A physician can no more check or cure an essential fever than he could stem a torrent with a pitchfork; and he who has not learned this before he undertakes the treatment of fever, may soon learn it at the fearful cost of his patient's life, and discover

that the pure Nosologist's dogma, "Show me a disease, and I will show you a medicine to cure it," is as untrue as it is presumptuous and unscientific. And yet a physician can do much in fever, and be like the careful mariner, who, understanding his ship, and consulting his charts and compass, can guide his vessel through shallows and sunken rocks, through storm and tempest, to the haven where he would be.

In fever the vital powers are depressed, the centre of circulation is weak, there is a tendency to death by syncope. Let him support his patient's powers, and by food and stimulants restore his failing strength, and he has accomplished much; and although not a single dose of medicine may have been administered to combat the poison of the fever, yet, when crisis is safely tided over, that physician has much more truly cured his patient and saved his life than a medicine-man has cured constipation by a dose of salts.

Again, as pain and discomfort are the exponents of the want of ease and rest, so the physician who can restore these does much to cure his patient; and as the delicate plant, withering and fading from the want of sun-light and fresh air, and from an obstruction to its growth by some obstacle in its way, revives and strikes forth in its full life and vigour, on the removal of the obstruction and the free admission of air and light, so the condition of the patient referred to can be readily turned from one of danger and unsafety to one of content and health by often the very simplest means.

Sedatives, in my opinion, based though it be upon a limited experience, but still such as to warrant me in that opinion, are means, though so often neglected, not only of aids to cure, but, more than any other class of treatment, curative in themselves. I could adduce, if necessary, many cases in furtherance of this view, but I will confine myself to one instance, where the hypodermic treatment of morphia checked, though it could not cure, the ravages of disease. The case was that of a man who was under Dr. Stokes' care, in the Meath Hospital, suffering from abdominal cancer. The details of the case are published in the Proceedings of the Pathological Society of Dublin, Vol. IV., Part III., p. 290. For weeks he had been unable either to lie down for sleep, owing to the distended state of the abdomen and to the constant and intense agony he suffered, or to take any food without its being ejected in an hour, with renewed pain; but, after that the hypodermic use of morphia had been regularly employed (sometimes it was needed three times a day), he seemed literally to improve, his appetite returned, the vomiting



ceased, and a visible and palpable alteration for the better took place in his general condition for a period of some weeks.

Sedatives directed to the cutaneous nerves are a means, I conceive, of very great importance, whether it be employed by inunction of sedative medicine, or by soothing applications. I cannot refrain from again quoting from the lessons, I am glad to say, I learned in the County of Dublin Infirmary; and I shall allude to an instance where this treatment of the peripheral nerves was employed in one of the exanthemata in a form somewhat unusual; and where, without exaggeration, a bath was the salvation of the sufferer. I do so the more readily as the case occurred in a fellow-student of my own, and as I was an eye-witness of its beneficial effect, while acting as clinical clerk. I refer to it also as the case has been published by Dr. Stokes, in furtherance of the advantages of soothing the cutaneous nerves when in a state of pain and over-stimulation:—

“Not many years since,” writes Dr. Stokes (*Dub. Journ. of Med. Science*, 3rd ser. p. 13, 1872), “one of our students, a very large and robust man, was attacked with small-pox, which soon showed itself in its worst characters. The fever at first was very high, and the head swelling and vascularity of the face intense. The eruption was universal; while the pustules on the face became confluent at an early period. Delirium set in, and the patient tore off the dressings from his face so often that we desisted from their further application. After the tenth day the condition of the patient was most appalling. The delirium continued, the circulation became every day weaker and more rapid, notwithstanding the free use of stimulants; the crusts were not only black, but on the legs, where, here and there, there was less confluence, the blackness of the worst purpura appeared—a condition held by Hebra to be always fatal. The body was one universal ulcerous sore, and the agonies of the patient, from the adhesion of the surface to the bed-clothes, were not to be described. In addition to the usual fœtor of small-pox in the stage of decrustation, which was present in the highest degree, there was an odour of a still more pungent and offensive character, which seemed to pass through a by-stander like a sword. I never before or since experienced anything similar. Stimulants alone, freely and constantly employed, served and preserved this patient alike; the pulse was rapid, weak, and intermitting, and, for several days, we despaired of his life. At this juncture I happened to describe the case to my colleague, Dr. Smyly, who suggested the trial of a warm bath, with a view of relieving the terrible

suffering. A bath, in which he could recline, was speedily procured, and pillows being adjusted in it, we lifted the sufferer in and placed him in the recumbent position. The effect was instantaneous and marvellous. The delirium ceased as if by magic; it was the delirium of pain, and the patient exclaimed—‘Thank God! thank God! I am in heaven, I am in heaven! Why didn’t you do this before?’ The fœtor immediately and completely disappeared, so that on entering the ward no one could suppose that there was a case of small-pox in it. He was kept at least seven hours in the bath, during which time brandy was freely administered, and omitted only when he showed symptoms of its disagreeing with the brain. He was then removed to bed. The surface was clean, and in many places the sores looked healthy and white. The bath was repeated next day, after which he fell, for the first time, into a tranquil slumber. From this time his recovery was progressive, delayed only by the formation of abscesses and the great soreness of the feet. That this gentleman’s life would have been sacrificed but for the timely use of the bath, few who have had any experience in prognosis can reasonably doubt. He was in the condition of a patient every portion of whose skin had been burnt and ulcerated.”

In the treatment of convalescence from fever, or, indeed, from any acute disease, the principle of rest is most important, and many a case is thrown back by a heedless disregard to its precepts, and retarded by undue exertion of body or mind, or both. Hence must be impressed the law—equally important in a hygienic and therapeutic view—that, in the recovery from any illness, long or short, exercise, to be beneficial, must be accurately proportioned to the strength of the individual, and must never be carried to the extent of actual fatigue or temporary exhaustion. This over-exertion of the brain or mind is by no means an uncommon cause of delay in the convalescence from fever amongst the better classes, as, too often, the interest in the subject-matter of some entertaining book overrides the patient’s prudence, and thus a state of sleeplessness and nervous excitability is readily produced. Remove the exciting cause and soothe the condition by some nerve sedative, such as the bromides of ammonium or potassium, and how rapid the change! how progressively the improvement will continue!

The following instances, in which this agent became a very important therapeutic, are taken from among the cases which came under my notice in private practice, and in the necessarily limited range of cases from which I could make a selection must rest my

excuse, if such be needed, for their want of numerical sequence and for the diversity of application—and yet, it occurs to me that this very diversity may serve my purpose better than if I were to adduce a considerable number of instances of the same disease treated by the same remedy, as it is more the wide principle than the detailed example; the indication for treatment, based on sound physiological views, rather than the treatment itself, which it is my wish to advance. I have not hesitated in one or two instances, whenever it occurred to me necessary, to avail myself of some striking examples, from the writings and experience of others, in furtherance of these views, and for the better elucidation of my subject.

In considering the circulatory system in connexion with the question before me, I shall allude briefly to its application in inflammation of the pericardium. The treatment of this disease, whether it occur in rheumatic fever or as a complication of other morbid conditions, diathetic or specific, by antiphlogistic remedies and lowering diet, is now, I trust, a thing of the past, while the treatment by opium, so prominently and truly urged by Dr. Stokes,<sup>a</sup> is that which is almost universally adopted. This treatment well illustrates my position. The drug acts here, not as a soporific, as the large and oft-repeated doses, which are tolerated by patients suffering from this affection, show, but its influence is seen by quieting the action of the heart, relieving pain, and checking over-excitement of the organ, and thus, by giving physiological and natural rest, it becomes a therapeutic, unrivalled in the universality of its application and in the certainty of its results.

Again, in the stage of serous effusion—the period of the disease when the patient's danger is great and imminent, when the pressure of a copious and rapidly-effused fluid hampers the free action of the ventricles, and clogs the more feeble auricles, and when the heart, in its endeavour to overcome all these difficulties, works itself, so to speak, into a state of irritability and consequent want of rhythm in its proper sequence of contraction and dilatation—at such a time is rest urgently called for; for should it not be soon obtained, it is certain its feeble powers will become still more feeble, and the patient will sink and die from an exhausted heart. How, then, under these circumstances, can the physician best attain this object? The treatment cannot, of course, be the same as in the former case. To quiet the heart by opium is to hasten the patient's death. Hence the line of treatment must be, in the first

<sup>a</sup> Diseases of the Heart and Aorta, Dr. Stokes, 1854, p. 91.

place, by calling into action the skin, the bowels, and kidneys, so as to produce the absorption of the effused and strangling products of inflammation. These means usually succeed, but should they fail, he can act by mechanically reducing the pericardial enlargement by tapping. This operation of paracentesis pericardii was originally suggested by Senac, and practised by Desault at first, and since then by Trousseau and others, both abroad and at home. The operation, while in many instances it has failed to give permanent relief, has fortunately been occasionally of the greatest service; not alone as a palliative of the sufferings of the patient, but, by resting the overburdened organ, it has restored that equilibrium of force to resistance, of supply to demand, which is so much needed. This is well illustrated by a case described by Dr. Clifford Allbutt.<sup>a</sup>

The beneficial action of direct cardiac sedatives or stimulants, according as the action of that organ be excited or depressed, is too well known and generally acknowledged for me to lay stress upon their employment further than to claim their evidence in favour of the view I maintain.

If, then, as I have attempted to show, rest be necessary for the welfare of the heart and the more perfect execution of its work, when its action is impeded by pericardial effusion, it is no less loudly called for, and is no less practically useful, in cases of organic disease affecting the heart itself, its walls, the orifices of its cavities, or its valves.

Let us consider its applicability in one of valvular disease—say of the mitral orifice, either stenosis or patency. So long as the equilibrium of muscular force to the resistance in the current of the blood be maintained, whether the resistance be by mechanical obstruction at the valvular opening or by unequal distribution of the contents of the left ventricle, as occurs in regurgitation, little inconvenience may be experienced by the patient, but as soon as this state of healthy action is thrown out of balance, and the obstruction is not overcome by the powers of the heart, so soon does the heart stand in urgent need of other help, and of physiological rest to enable its restoration to the normal condition.

The physician, while powerless to restore organic alterations of the valvular apparatus, can do much to relieve the immediate distressing symptoms, and to reinstate the organ in some degree to its former position and muscular power. And here I would digress

<sup>a</sup> Brit. Med. Journ., p. 384, October 10th, 1868.



for a moment from the immediate object of my paper, to remark that in the diagnosis and treatment of diseases of the heart, too much stress is often laid upon the condition of the valves—as to whether they cause obstruction or regurgitation—and too little importance given to the all-important consideration of the muscular tissue, whether it be strong and efficient, or weak and yielding, and yet it is this latter consideration which is the surest guide to a judicious and successful line of treatment.

The cyanotic aspect of the face, the coldness and œdema of the extremities, the distended and pulsating jugulars, the semi-comatose or lethargic somnolence, the labouring heart, with its right side widely distended with blood, the congested lungs, the functions of the liver, stomach, kidney, and skin in abeyance, present a group of symptoms too plainly indicative of danger, and demanding immediate relief. Rest, in the widest signification, must be obtained; for if the burden be not lightened, or the patient and hard-working organ be not strengthened, the latter will fall beneath its load, and death by syncope or asphyxia be the result. The physician's means are two-fold to attain this end. One is by directly diminishing the amount of fluid circulating, and thus to lighten the right side of the heart. The other, by strengthening and regulating the action of its muscles, to enable it to overcome the obstruction to such a degree as to avert, for a time at least, the fatal result.

The first object is obtained by venesection, wet cupping, and by purgatives, diuretics and diaphoretics. In many cases the latter will suffice, but not always, and then it will be necessary to employ venesection to several ounces.

I believe, and in this view I am borne out by no less an authority than Dr. B. W. Richardson, that venesection is a means of paramount importance, and one too often neglected or delayed, until by over-distension the right ventricle becomes too weak and dilated ever to recover its normal condition. The means of strengthening the muscular powers of the organ are by general tonic and stimulant treatment, and more particularly cardiac stimulants. At the head of this latter list stands digitalis, as being the most active and best known of this class. By the employment of this drug we obtain a two-fold means of placing the organ in a state suited to meet the demands upon it in mitral disease, in addition to its stimulant qualities. By the physiological action of the drug the number of the beats of the heart are reduced, sometimes by forty in the minute, and while the ventricles are acting more forcibly, the blood, which

before stagnated, so to speak, in the right ventricle and pulmonary arteries, while the left ventricle had hardly enough to fill it, can now pass through the narrowed opening during the diastole. Thus, while the right side is less oppressed and its cavity diminished, the left ventricle can propel through the general systematic circle a fuller and richer stream. The action of digitalis in restoring a balance of the pulmonic and systemic circulation, has been so fully entered into and discussed by Fothergill,<sup>a</sup> in his treatise on the action of this drug, that it is unnecessary further to delay in commenting on this subject. The following case well exemplifies the foregoing:—

CASE I.—*Mitral Stenosis; Hemiplegia; Venesection; temporary recovery.*

M. Crystal, aged twenty-two, a tall, slight-built young man, came under my care, in connexion with an educational establishment of which I had the medical charge, in the beginning of 1872, complaining of slight cough and palpitation. On 9th January he got up in the morning, as usual, but not feeling well he lay down again, and, while the servant was in the room, he became unconscious and fell in a fit of apoplexy, which lasted about an hour. When I saw him, about two hours after this attack, he was hemiplegic on the right side, with aphasia and loss of sensation; the heart was acting most irregularly and rapidly, so much so that it was almost impossible to count its beats or to say which sound was the first; a rough sound was audible at apex, but whether it was systolic, diastolic, or pre-systolic, it was impossible to say while the heart was so excited; the left side of the body was constantly in motion, of a choreic nature; this symptom was particularly noticed in the left leg, which was incessantly in motion. I gave him digitalis combined with liq. morph. acet. in small doses. The next day he seemed improved, speech was better and pulse quieter, but during the following night he became extremely excited, delirious, and endeavouring to get out of bed. As his condition was one of great danger, and he required attention which could not well be given him in the institution, Dr. James Little kindly admitted him to the wards of the Adelaide Hospital, where he received the greatest care and attention that skill and kindness could afford. Owing to Dr. Little's kindness I was permitted to continue my observations of this very interesting case. It became now evident that the apoplexy and paralysis originated

<sup>a</sup> Digitalis, its Mode of Action and its Use. By J. Milner Fothergill, M.D. 1871.

(without any history of rheumatic fever) in mitral stenosis, and that a portion of fibrine, becoming detached from the valve, was carried in the current of the blood and lodged as an embolus in some artery on the left side of the brain, most probably the middle cerebral. By percussion it was evident the right side of the heart was unduly loaded, and the pulmonary vessels full. Dr. Little accordingly bled him from the arm, to the amount of 10 $\bar{3}$ , and to a similar amount on the third day of his residence in hospital; at the same time he was put upon full doses of digitalis. Under this treatment the overloaded and excited heart acted more regularly and slowly, and the patient was brought out of imminent danger. The paralysis, which was probably due to temporary anæmia rather than pressure of a clot, or laceration of the brain substance by hæmorrhage, became rapidly better; the leg could be moved on 14th January, and before a month elapsed the patient was able to sit up and go about with a stick. He afterwards went to the country, and lived there for several months.

In this case, thus briefly summarised, the balance of power to the work to be done could only have been maintained by the direct depletion which was practised, and thereby the right ventricle, aided in its powers of contraction by the use of digitalis, was saved from the paralysing effects of over-distension.

The beneficial influence of rest upon the heart is not more striking when the cause lies in weakness of the heart's walls, or in an obstruction at its orifices, than when the organ becomes affected secondarily to retained excretions in the blood, and the emunctory functions of the kidneys have temporarily failed. In these cases of acute general anasarca, following upon acute nephritis, or acute congestion of the kidney, there is a twofold danger of death, beginning (1) in the heart; (2) in the brain. If these cases be seen soon enough, there is no remedy in the physician's hands so rapid in its action, or so beneficial, as the relieving the system generally, and the right heart in particular, of a portion of the circulating fluid. Two months ago a case came under my notice which strongly impressed me with the truth of this—and although the patient, owing to the reduced circumstances of his family, passed out of my hands, and for some days after was in a state of great peril—the treatment I adopted sufficiently impressed me, and convinced me, that to venesection the patient owes his life.

The following are the particulars:—

CASE II.—*Acute Congestion of the Kidney; Suppression of Urine; Convulsions; Mania; Venesection; Recovery.*

William Clegg, about fifty-five years of age, was exposed to prolonged wet and cold on the 1st of October last, and felt very unwell in the evening; for some days he continued his occupation of van-driver, although becoming each day more unwell, and while his face was noticed to be swollen. His urine diminished in quantity, and for a day or two he remarked it of a darker colour, but, he stated, it never was bloody or like porter. There was some uneasiness in his back complained of, and a frequent desire to micturate. As the dropsy had increased to his abdomen on the 4th, he kept his bed, and I saw him in the afternoon of the 5th. His condition was then as follows:—Face puffed; eyelids and conjunctiva œdematous; whole body and legs swollen; but the swelling was confined more to the cellular tissue than the serous cavities, and there was no swelling of penis or scrotum. He was unusually drowsy and heavy; much thirst had been complained of, with complete loss of appetite; and the bowels had been costive, and were acted on once the day before by a dose of salts. Pulse was slow, 60, but very full and incompressible. Percussion showed an increased area of cardiac dulness, and partial dulness in the base of both lungs—due, no doubt, to serous effusion into the pleura or base of lungs.

At my request he passed some urine, of a deep sherry colour, acid, albuminous to a fifth, specific gravity 1,020, and presenting, under the microscope, a great number of blood discs, and some casts of renal epithelium, with a few spermatozoa.

I ordered 30 gr. doses of comp. powder of Jalap with gr. 3 of calomel, every fourth hour, till effect should be taken on the bowels. In six hours afterwards there were two large motions, but not particularly watery. As he was returning to bed a new train of symptoms appeared; for he was attacked by an epileptic seizure, during which he bit his tongue, and, as I was informed, worked a great deal. This was followed by others, at first every two hours, then every half-hour, and when I saw him, at 11 p.m. that night, he had a fit during my visit. All the stages and features of an epileptic seizure of the *gravior* type were well exemplified. After the period of sopor, moreover, which was very prolonged, he could hardly be aroused, and seemed not to recognize the bystanders, while the purple aspect, which the face assumed during the second



stage, did not subside in the intervals of the attacks. The pulse rose to 80, and seemed very laboured. A third powder was now given, but the bowels were not moved by it. The attacks soon became more frequent, and changed somewhat in character, and between the paroxysms there was complete unconsciousness, so that no food or drink could be taken. The night was a most wretched one, as during the whole of it he was incessantly in motion, his hands were passed or rubbed up and down the abdomen, and across the chest, with what seemed to be a designed regularity, so that no dress or bedclothes could be kept on him, while he moaned and rolled his head from side to side. The only momentary arrest of this movement was caused by a fresh attack of convulsions, and lasted but the few seconds of the first stage of tonic spasm. These latter changed from being decidedly of the "gravior" to the "mitior" type, or, as his friends said, "the fits became more internal," being expressed less by the movements of the extremities than by the suffocation and difficulty of respiration. In the morning, and during the forenoon of the 7th, they became still less marked, and escaped notice, so much so that his wife thought he was better; but to a careful observer it was evident they were just as frequent; for, during my visit, he had a couple of slight attacks in the period of fifteen minutes, which was recognizable only by the fixing of the eyes, insensibility of the eyes to light and objects, and temporary arrest of the respiratory movements. This train of symptoms would then pass off, and the restlessness and moaning return.

The swelling had now greatly increased; and although there was no urinous odour exhaled, the kidneys were failing to secrete the urine, as the bladder was empty. The pulse became small, compressible, and irregular; the breathing shallow and short; the venous congestion of face, eyelids, and neck, intense. This group of symptoms pointed plainly to arrest of the free passage of blood through the lungs, and to an over-distended right heart, while the kidneys failing, from, no doubt, extreme venous congestion, to eliminate the fluids and poisonous constituents circulating in the blood, presented a sufficiently bad combination as to warrant me in holding out the worst prognosis possible. I, however, urged the necessity of bleeding him, as, until some of the overplus could be removed from the right heart and general circulation, the kidneys could not regain their excretory powers. After some difficulty I obtained sanction to operate, and forthwith I allowed about 12 ounces of blood to flow from the arm, being guided more

by the general aspect of the patient than the character or frequency of the pulse. While the blood was flowing the intense purple of the forehead and face became less; the lips became red instead of blue, and the relief was evidenced by the deeper inspirations and occasional full cough; the restlessness also diminished, and the rubbing of the hands ceased. Pulse full—72. Hot stupes were now applied to loins, which were freely dry-cupped. I advised the comp. pulv. jalapæ to be renewed, and gave a mixture containing tinct. and infus. digitalis, spir. junip., and spir. æth. nitrosi.

Only one fit occurred for four hours after venesection was practised, and the medicines, beef-tea, and gin were taken, and in nine hours he became quite conscious, and recognized his wife. Three large watery motions came away in the night, during which he slept fairly, all the restlessness having ceased. On my morning visit on the 8th, I was agreeably surprised to find him sitting up in bed, propped up on pillows, and, although rather confused and dull, he recognized me. He complained of great stiffness and soreness all over his body, evidently that of muscular pain due to the great strain they had undergone. Pulse 60; respiration full and easy. The swelling had greatly gone down, not only from the face, but from the feet and legs also. The digitalis was now omitted, but the gin (two glasses) was continued. On 9th the dropsy was entirely gone, and he seemed much more rational; there was, however, an unusual degree of irritability in his manner, and his mind seemed inclined to wander. He sat up at the side of the bed at my request, and passed a little water. It was darker than before, but clear. It was albuminous, and contained a greater number of free blood corpuscles, and casts, both large and small, of the tubuli uriniferi, some epithelial and some granular. His pulse was very slow—48 in the minute.

As the means of the family were much straitened, and I thought better care could be paid to his wants and his state, which was still one of danger, in an hospital, Dr. Foot kindly admitted him to the Meath Hospital on the afternoon of the 8th. For some days after admission he presented very alarming symptoms; for, in addition to the convulsions, which returned, as I am informed, on the night of his admission, he became quite out of his mind, and required to be mechanically restrained in a separate ward. I am glad, however, to say that lately—December 7th and 9th—he has called to see me in my study, seeming quite well; and his urine was normal in most

characters, although in greater quantity than usual. He has since resumed his occupation of van-driver.

The same line of treatment, though to a less degree, is alone applicable in many other affections of the heart. In the case of a weak dilated heart, the patient is usually greatly oppressed by the effects of the dropsy, which, sooner or later, appears, its presence causing an imperfect action of the liver and kidneys. In these cases the physician, to enable the heart to carry on its functions, usually employs another means to relieve the patient, and rescue him, at least for a time, from a condition of imminent peril to one of comparative and real safety. The ordinary diuretics too often fail to remove the fluid, which steadily, slowly, but surely increasing, threatens to hamper and strangle the centres of life. The line of treatment to be adopted, however hard it may be to explain its *modus operandi*, is one of the greatest importance, for by the administration of *mercury* alone can the rest, which the heart so much requires, be obtained, and the dire results averted. This, Dr. Stokes was the first systematic writer to call attention to, and I cannot refrain from quoting his experience on this subject, delivered in his own graphic manner. He writes,<sup>a</sup> speaking of cases of weak dilated heart:— “These patients are from time to time liable to the occurrence of dropsy, which, commencing in the extremities, will, if not soon checked, invade the entire system. This condition is preceded by diminished action of the kidneys; the heart becomes more excited and irregular; the liver swells like an erectile tumour, and the lungs are oppressed by congestion. Orthopnœa is established, attended with the worst paroxysms of cardiac asthma. Ascites may appear, and the patient’s life be threatened daily. Yet under all these terrible symptoms, it happens again and again that the exhibitions of mercury will, as if by enchantment, remove the anasarca, reduce the hepatic tumour, restore the heart to its ordinary, though not its normal condition, and for a period of time, more or less long, enable the patient to pursue the avocations of an active and laborious life.”

The following case will exemplify this rule, and, further, is an excellent illustration of the beneficial results of the employment of direct anodynes and soporifics, especially when employed hypodermically:—

<sup>a</sup> *Loc. cit.*, p. 354.

CASE III.—*Weak dilated Heart ; Emphysema ; Enlarged Liver ; general Anasarca ; Cardiac Asthma ; treatment by Mercury, and hypodermic injections of Morphia ; Recovery for two years.*

Mr. H., a large, full-bodied gentleman, aged seventy-two, came under my care, in connexion with other physicians, in the autumn of 1870. His condition, when I saw him, was most distressing and alarming. He lay propped up in bed, unable to lie down, his lips and face purple, with the veins standing out on his forehead and neck like cords ; his body, scrotum, and legs anasarous ; his hands blue, cold, and swollen ; his respiration loud, gasping, and shallow. Pulse small, irregular, and intermittent ; urine diminished in quantity and albuminous ; hepatic dulness greatly increased. I learned he had been in this condition for over a week, and had had during this time no regular sleep or rest by night or by day, and that on occasions he had paroxysms of dyspnœa and coughing which left him almost dead. On Dr. Hudson's advice, to whose kindness I was indebted for having the charge of this case, he had been taking pills of hydrargyrum and iodine, and a mixture containing digitalis and liq. strychniæ. At first I attempted to procure sleep by the free administration of chloral hydrate ; while I urged the persistent employment of the mercurial course to touching the gums. The chloral failed to procure anything like natural rest, tending rather to excite than soothe, although given in very full doses. And here I would venture to remark that this drug, so useful in many other cases, has—on several similar occasions of weak heart—failed to give satisfaction in my hands. I now determined to employ the hypodermic use of morphiæ acetat, and as I noticed, that at certain hours of the day and night this gentleman's sufferings were much aggravated, and the oppression to his breathing became most intense, so, much so that the gasping for breath was most distressing to witness (true cardiac asthma), I arranged to inject one-sixth grain, about an hour previous to the time of wonted aggravation. This line of treatment, suggested to me by the periodicity of the attacks of dyspnœa, was marvellously successful, and far surpassed my most sanguine expectations ; for not only did the patient, in less than seven minutes, fall into a quiet slumber, but the paroxysms became retarded, and by persistence in this method they became in a short time less severe in character and duration. In less than four days the signs of evident improvement became apparent, and a single hypodermic dose of one-third



grain of morphia at bed hour sufficed to secure a tolerable night's sleep, and a quiet day following.

It is enough for my purpose to state that under the steady employment of the mercurial treatment, which was pressed until the breath became markedly affected and the gums spongy, the dropsy diminished, the cough and frothy expectoration became less, the hepatic tumour diminished, sleep was restored; and in four weeks this patient was able to undergo a journey of a carriage drive of fourteen miles, and three hours by rail. By change of air, careful regulation of diet, and avoidance of all causes, physical and moral, of excitement to the heart or lungs, he was able to enjoy life for nearly two years afterwards. On three occasions during these twenty-one months, a recourse to the same line of treatment was required, with a like happy result; but at last a time arrived when the system no longer responded to the action of medicine, and the patient sank with dropsy and pulmonary congestion.

In this instance rest natural and rest physiological combined, succeeded, when one alone would, beyond all doubt, have failed.

I cannot leave the subject of "cardiac rest" without adducing another example, in which, in a most marked manner, this agent was imperatively called for, and in which it was followed by almost instantaneous relief. The case was, so far as my limited experience goes, a very peculiar one, and I would also judge it to be so from the very imperfect description of any similar case I have been able to discover through the writings of many authorities on Diseases of the Respiratory Organs.

CASE IV.—*Nervous Asthma; Pulmonary Stasis; treated by chloroform inhalations.*

The case, which came under my notice and care in 1863, and was under observation for many years, was that of a young lady aged twenty-four, highly gifted, and of a peculiarly sensitive and quick disposition. From her childhood she was subject to attacks of bronchitis, or rather paroxysms of cough and dyspnoea, which ended in copious expectoration of frothy mucus. These asthmatic attacks became more frequent as she advanced through puberty, and were always accompanied with, or followed by, the most violent palpitations. During these paroxysms, which continued sometimes for a week together at the time I refer to—in 1863—with intermissions, or, more correctly speaking, remissions of

a few hours' duration in the middle of the day, her state was a most pitiable one, and her appearance not less so. Her lips, ears, alæ nasi, and fingers were blue, cold, and pinched; her ankles and feet swelled; the power of concentrating thought gone, while head-ache and somnolency presented a sad contrast to her usual vivacity and spirited powers of conversation. Her breathing, which was loud and audible all over the house, and accompanied with a wheezing noise, ranged from 30-40 in the minute, while the pulse varied from 110-130; and the palpitations could be easily seen and felt through the bed-clothes covering her. All through, there was an absence of fever; and, strange to say, as soon as these attacks passed away, or rather were passing away, she would get out of bed, and would go out driving in the open air, no matter whether it was dry or wet, hot or cold, and nothing seemed to hasten her recovery towards the close of the attack so much as driving rapidly through the air, with the wind blowing her hair freely about her head, which very often was uncovered. Taking into consideration all the circumstances of the case, and the absence of all physical signs of organic disease in lungs or heart during the intervals of health, when she would endure fatigue and undergo more exertion than many of her sex in her station of life, I concluded that the error lay rather in the nervous system and the nervous government of the heart and lungs, than in organic disease of the latter. Acting on this view, I believed that to quiet the heart by relaxing the spasm of the bronchi, and thus to unload the right chambers of the organ, which was manifestly in that condition, was the direct indication of treatment. Rest was needed, and, if the right side of the heart could be emptied, I had little doubt the palpitation, which was so dreadfully distressing a symptom, would cease also. Accordingly, I advised the immediate employment of chloroform inhalation, and superintended its administration myself. The effect was almost magical. Hardly were thirty drops inhaled than the breathing became fuller and slower, and the heart's beats fell in number and violence. This treatment I continued to employ for about ten minutes, with intervals, and could hardly restrain the patient from employing a dangerous quantity of chloroform, such relief did she feel. At the end of this time her condition was wonderfully altered, and from a state of gasping orthopnoea and the condition already described, she sank into a placid state, with the feeling of the most intense relief.

This treatment I had on several occasions to repeat, while she

was under my observation, with the happiest results. It is not to the purpose to dilate upon this case, as it is to the principle of rest I wish to call attention, and to the many and varied means, often the most opposite, we must be prepared to adopt, to attain this object.

The last example of the inestimable advantages of rest which I shall adduce, and I know no more forcible example, is the treatment, on this principle, of internal aneurism. It combines rest physical and rest physiological. Originally instituted by Valsalva, it has been most admirably carried out to a successful issue by Mr. Joliffe Tufnell of this city. This gentleman, ten years ago, published cases illustrating the treatment and cure of this most fatal disease by this grand therapeutic, laying more stress, than Valsalva did, upon the necessity of position and a fibrinous diet, and less upon the repeated bleedings which Valsalva chiefly depended upon.

Lately, Mr. Tufnell has had opportunities of examining the tumours which, years ago, had been cured, and has had the gratification of exhibiting the specimens, including one of aneurism of the abdominal aorta, before the Pathological Society of Dublin, as well as the Medico-Chirurgical Society of London.

By his persistent adherence to this principle, and by unswervingly following it out in its minutest details, he has added a fresh laurel to the Irish School of Surgery, already so famed throughout the world for the surgery of arteries.

As it is not my intention to discuss Mr. Tufnell's modification of Valsalva's treatment, or to compare it in its application or results with that treatment, I will merely refer to its *modus operandi*. The treatment consists of three parts—first, rest of the body in the horizontal position for weeks or for months; second, the diet limited and restricted to fibrine-making food; and, third, venesection, local or general. Of these, the chief factor is, I believe, the first, which acts both mechanically and physiologically—mechanically by arresting all muscular exertion, and physiologically by favouring coagulation of the blood in the aneurismal sac. This latter result is no doubt aided by the food being such as to increase the proportion of fibrine to the other constituents of the blood, but position is, I conceive, the most important.

This can best be understood if the pulse-rate be observed in the erect position and in the horizontal. Drs. Graves and Guy have

shown the effect of position on the rate of the pulse, in health, to cause a variation of ten to twelve beats in the minute, while occasionally it may be so great (as in one of Mr. Tufnell's cases), as thirty in the minute.

Thus, while in health the heart impels the blood through the arteries 14,400 times oftener in the upright position (when at rest) than in the lying in twenty-four hours, the vessels may be distended 43,200 times, in the same time, in disease. The benefits of rest, therefore, as the best possible means of placing the patient on the way to nature's cure, is self-evident, and needs no further words of mine to impress its advantages. All that can be said in its favour has been said by Mr. Tufnell, and no stronger witness can be borne to its efficacy than that shown by the results.

In the foregoing cases, which might be multiplied *ad infinitum*, both as to number and varieties of application, I have attempted to illustrate a principle, not to originate a treatment; to widen and strengthen the foundation of physiological and rational medicine, not to erect a superstructure; and to call attention to a great therapeutic—applicable to every case, medical and surgical alike—not to laud any specific medicine.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

*A Practical Treatise on the Diseases of the Heart and Great Vessels, including the Principles of their Physical Diagnosis.* By WALTER HAYLE WALSH, M.D., Emeritus Professor of the Principles and Practice of Medicine in University College, London. Fourth edition. London: Smith, Elder, and Co. 1873. 8vo, pp. 582.

IN Dr. Stokes' great treatise, well termed by Dr. Walshe a classical work, we are supplied on all the questions of which it treats with the mature opinions of one of the most philosophic physicians who has ever devoted attention to cardiac diseases; and we have other valuable monographs on the same department of our science; but the book before us, dealing with the entire subject, has for many years been regarded as the most complete treatise on diseases of the heart and great vessels, and we can confidently affirm that the present edition, giving, as it does, precise information as to the clinical topography and physical examination of the heart, discussing all questions of cardiac pathology in the light of recent researches, supplying full details as to treatment, and referring, with scrupulous care, even to the rarest lesions, fully maintains the high position accorded by universal consent to those which preceded it.

Since the publication of the last edition considerable attention has been paid to mitral regurgitant murmurs of dynamic mechanism. Dr. Hayden, and, more recently, Dr. Nixon, have added much to our knowledge of the conditions under which this murmur occurs; and the latter contributed a valuable critical examination of the theories which have been advanced to account for it to the June number of this Journal. The differential diagnosis between mitral murmur due to structural change in the valve and mitral murmur of functional mechanism, involving, as it does, difference in prognosis and in treatment, is a matter of great importance. The conditions under which the functional murmur occurs are divisible

into two groups; in the first, though there is no structural alteration in the segments of the mitral valve, or puckering or other change in the chordæ tendineæ which draw them together, there is organic change in the left ventricle, its walls are dilated or have undergone softening or fatty degeneration; in the second group we find cases in which the heart is texturally sound, but in which, apparently from some error of innervation, the muscular fibres fail to contract with that precision necessary for the perfect closure of the valve—such are cases of chorea and epilepsy, and of persons whose nervous system has been profoundly injured by masturbation or smoking. Setting aside the assistance we may get from the circumstances under which we encounter the murmur, can we determine its organic or functional character? The attributes of the murmur indicative of regurgitation through the mitral orifice, consequent on textural changes in the valvular apparatus, are these—it is systolic in time, is of greatest force at or about the apex, and is audible in the axilla, at the inferior angle of the left scapula, and in the groove between the scapula and the spine. Moreover, in a certain proportion of cases of mitral reflux, the overfilling of the left auricle and of the pulmonary system of veins opposes such an obstacle to the current of blood from the right ventricle that the tension in the pulmonary artery is materially increased, and, in consequence, the vibration of the sigmoid valves at its mouth produces a sound of greater intensity than that which is developed by the vibration of the valves at the mouth of the aorta, and we hear the second cardiac sound with greater distinctness at the second left (pulmonary) cartilage than at the second right (aortic) cartilage. Now, according to Dr. Walshe, the most important of these signs, in respect of the question now before us, is the audibility of the murmur at the inferior angle of the scapula and in the vertebral groove. “If,” says he, “the murmur be imperceptible posteriorly, I believe it *very seldom depends on regurgitation of structural mechanism*; possibly it may *never* do so, but I am unprepared to adopt so sweeping a formula. Still I think we may say it *never depends on well-pronounced organic regurgitation.*” Dr. Nixon, in the paper to which we have already referred, states a different opinion. He believes that reliance cannot be placed on the absence of murmur behind, and attaches more importance to intensification of the murmur in the recumbent posture, and absence of accentuation of the second sound over the pulmonary artery, as characteristics of the functional mitral murmur.

Another subject to which much attention has been paid, and on which there has been a great modification of opinion since the appearance of the third edition of Dr. Walshe's Treatise, is the action of digitalis, and the indications for its use in diseases of the heart. Convinced as physicians have been for the past hundred years that it possessed unrivalled powers in relieving many kinds of cardiac suffering, they were also convinced that its administration sometimes led to the most disastrous results; and it is a point of the first importance to determine wherein lies its power for good, and wherein lies its danger. Its effect in slowing the action of the heart is one about which no question has arisen, but the views formerly held as to its further action have been materially modified. In the fourth edition of Pareira's *Materia Medica*, published in 1855, its power of *enfeebling* the heart's action is mentioned as one of its most valuable properties, but in recent works on therapeutics, its influence in *strengthening* the contractions of the ventricles is prominently dwelt upon; and, in consequence, physicians sometimes meet at the bedside imbued with entirely different notions as to the circumstances which justify its use. Believing, as we ourselves do, that digitalis would be much more accurately described as a cardiac tonic than as a cardiac depressor, we, nevertheless, are convinced that its peculiar action is not sufficiently indicated by that term, and we have read with great interest Dr. Walshe's opinions as to its properties and uses, written, as they have been, with full knowledge of the numerous experimental researches which have been made on the subject, and with accumulated experience of its use in disease. Collecting the various references which occur to the drug in the work before us, Dr. Walshe's views may, we believe, be embodied in the following propositions:—

There is considerable difference in the effects of digitalis on different persons, and even on the same person at different times.

The effects at first produced differ entirely from those which are observed after a long-continued use of the medicine.

At first (say in doses of five to fifteen minims twice or thrice daily) it slows the heart's action, increases the force and prolongs the duration of the ventricular systoles, and, if the contractions have been irregular, renders them regular. If a poisonous dose be given at the commencement of its administration, death results from tonic spasm of the muscular fibres. The dose which will be borne will depend on the previous condition of the organ; a heart contracting

frequently, incompletely, and irregularly, will be only made to contract slowly, firmly, and regularly, by a dose which would act poisonously on a healthy heart.

Moreover, it singularly relieves various distressing feelings about the heart.

If, however, its administration be prolonged, phenomena of an entirely different kind are exhibited—it accelerates the action of the heart, weakens, and renders it irregular.

In determining on the administration of the medicine, we should be guided less by the organic lesion present than by the state of the heart's action at the time; weak, *irregular*, and over-frequent action, more particularly if accompanied with uneasy sensations in the cardiac region, constitute the group of conditions under which digitalis affords unmistakeable relief.

Sometimes, however, it is useful in removing cardiac distress, even when the action of the heart is too powerful.

To guard against the ill effects which follow its prolonged employment, the heart's action must not be slowed much below the normal standard, the medicine must be omitted for forty-eight hours every third or fourth day, and it must be discontinued as soon as we observe any of the phenomena already mentioned as being produced by its too lengthened use.

The greatest danger seems to be that of too markedly slowing the ventricular contractions in cases of dilated heart, with or without valvular disease, in which there is a tendency to coagulation of the blood within the cardiac cavities. Our own experience leads us to believe that the use of the drug in patients with large flabby and dilated hearts requires especial care.

In the treatment of cardiac inflammation, both pericarditis and endocarditis, Dr. Walshe has quite abandoned the use of mercury.

The sphygmograph—perfected since the last edition of this treatise appeared—is fully described, and a number of tracings by Dr. Mahomed reproduced as photo-lithographs. “The instances, however,” says Dr. Walshe, “in which a writing of the pulse will of itself decide the nature of a doubtful cardiac or arterial affection, are certainly few in number. So far as it has yet revealed itself, the mission of the sphygmograph appears to be rather to indicate general conditions of the circulation (which may in the same form obtain in various and unallied diseases) than to detect specific organic changes. The identification of such general conditions may be greatly more useful as aids to prognosis than to diagnosis.”



### RECENT WORKS ON DISEASES OF THE SKIN.

*Skin Diseases: an Inquiry into their Parasitic Origin and connexion with Eye Affections; also the Fungoid, or Germ, Theory of Cholera.* By JABEZ HOGG. London: Baillière Tindall, and Cox. 1873. Pp. 108.

MR. HOGG's friends have made so many demands upon him for copies of various papers of his published from time to time in the *Quarterly Journal of Microscopical Science* and *Medical Times and Gazette*, that he has been moved to reprint the whole in a corrected form, under the above discursive title.

As a re-publication simply, these papers scarcely fall under criticism, and it is sufficient to say that from beneath a heterogeneous aggregate of incidental remarks, fragments of anatomy, accounts of experiments, &c., we discover the author's hostility to the generally received notions on the relation of the class of *Tineæ* in particular to fungi as their veritable cause; and he attempts to show that similar fungoid appearances are met with in various other affections not commonly admitted as parasitic, the appearance of the vegetation being simply dependent on pre-existing disease. We need hardly remark that it is one thing to discover spores in a number of diverse skin affections, and quite another thing to set aside the mass of positive evidence on the relation of certain definite diseases to fungi as their *bonâ fide* cause.

*Elephantiasis Græcorum, or True Leprosy.* By ROBERT LIVEING, A.M., M.D., Cantab. London: Longmans, Green, and Co., 1873. Pp. 150.

THE identification of true leprosy in different countries, and its discrimination from other disorders, such as the Barbadoes leg, syphilis, and scurvy, is beset with difficulties, and has only been satisfactorily accomplished within the last few years. In 1871 Dr. Liveing visited Norway, and had there ample opportunities of investigating this dreadful disease which, so far from receding in that country, is actually gaining ground, and spreading over districts where it was before unknown. Curiously enough, in Sweden it is fast dying out. By coupling his personal experience with a general survey of the literature of the subject, the author

has produced, in a concise and very readable form, an interesting account of this fearful malady, the very loathsomeness of which has for ages attracted to it an unusual amount of attention. The volume consists of the Goulstonian Lectures for 1873, revised and enlarged. Chapter I. sketches the history of leprosy during the middle ages. Chapter II., the geographical distribution of the disease in the present day. Its prevalence in different countries is most unequal, in some it is widely scattered, in others collected into foci. Chapter III. discusses the etiology of leprosy, at least as to its secondary and predisposing causes, for of its primary source we are completely ignorant. The last chapter treats of the clinical history, morbid anatomy, and treatment of leprosy. Unfortunately as to the latter point the verdict must still be—failure. No means of specifically influencing for good the progress of the disease has yet been discovered, and whether the duration of the malady be ten or twenty years, but the one sad result can be ultimately looked for.

*Photographic Clinique of the British Hospital for Diseases of the Skin.*

WE have before us Nos. 2, 3, and 4 of Mr. Balmano Squire's photographic representations of cutaneous diseases, and some time since we noticed, in this Journal, No. 1, which depicted true (Addison's) keloid. Photographic illustrations are, of course, perfectly accurate in outline; and Mr. Squire having been fortunate enough to secure an artist who colours truthfully and with good taste, the result is an admirable series of faithful pictures of some remarkable forms of diseases of the skin. We cordially recommend them to the notice of our readers; and all who are engaged in the special study or clinical teaching of cutaneous diseases will find these illustrations a most useful adjunct. No. 2 is a picture of rodent ulcer in a woman, aged 54, involving the left side of the nose and lower eyelid. No. 3 affords a life-like representation of serpiginous ulcerating syphilide on the back and right shoulder of a man, aged 68. Nothing could be more characteristic than the colouring of the greenish-yellow scabs seated on the irregularly ulcerated livid patches. No. 4 is a capital photograph of a middle-aged man, and illustrates a remarkable and extensive congenital vascular nævus of the face, complicated with large knobs of the so-called hypertrophic acne.

# PART III.

## HALF-YEARLY REPORTS.

---

### REPORT ON MATERIA MEDICA AND THERAPEUTICS.\*

By WALTER G. SMITH, M.D., Dublin; Fellow and Censor  
K. & Q.C.P.I.; Examiner in Materia Medica, Q.U.I.; Assistant  
Physician to the Adelaide Hospital.

#### ART. 12. Alkaloids.

- (a.) Reaction with sugar and sulphuric acid.
  - (b.) Quantitative estimation.
  - (c.) Aconite alkaloids.
  - (d.) Toxic effects of ammonium-bases.
  - (e.) Cinchona alkaloids.
- „ 6. Belladonna poisoning.
  - „ 3. Carbolic acid and zymotic diseases.
  - „ 8. Chloral.
  - „ 9. Croton-chloral.
  - „ 2. Homœopathic camphor, poisoning by.
  - „ 1. Homœopathic pilules.
  - „ 10. Nitrate of lead in onychia.
  - „ 11. Nitrite of amyl in epilepsy.
  - „ 7. Oxygen and ozone.
  - „ 5. Phosphorus, administration of.
  - „ 4. Senna—effects on urine.

1. *Homœopathic Pilules*.—In the April and July numbers of the *Practitioner* is recorded a series of chemical examinations of “pilules” of the second dilution, purporting to contain various active drugs. The average weight of the “pilule” was 0·6 gr., and each should accordingly contain 0·00006 grs. of the drug.

\* The author of this Report, desirous that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal they will be forwarded.

*Sulphate of Copper Pilules.*—Two samples, of 100 and 200 pilules respectively, were examined, but no copper whatever was detected in them, although if even so little as  $\cdot0001$  gr. of the sulphate had been present it would have been found.

*Corrosive Sublimate Pilules.*—It was just possible to detect mercury in 200 of the pilules. There was not  $\frac{1}{20}$  of the amount which should have been present.

*Nux Vomica Pilules.*—No strychnia could be detected, even when 300 pilules were employed, which should have contained about  $\frac{1}{10000}$  gr. of strychnine. Now, so small a quantity as  $\frac{1}{70000}$  gr. of this alkaloid can be distinctly recognised by chemical tests.

*Aconitum Napellus Pilules.*—No aconitia could be detected in two samples of 100 pilules each.

*Belladonna Pilules.*—No atropia could be detected in two samples of 100 pilules each. Comparative experiments showed that by means of chemical tests alone so little as  $\frac{1}{6000}$  gr. of atropia would have been detected with certainty.—(*Pharm. Journ.*, July 19, 1873).

2. *Poisoning by Homœopathic “Concentrated Solution of Camphor.”*—Dr. G. Johnson related to the Clinical Society three cases of poisoning by this preparation (Epps’), which is 7·2 times as strong as the spiritus camphoræ, B. P.

A young lady, aged twenty, having a cold, took twenty-five drops before going to bed. In a short time she was found foaming at the mouth, black in the face, and violently convulsed. She vomited blood-tinged fluid, smelling strongly of camphor, had severe gastric pain, remained unconscious for several hours, and was upset for six months afterwards.

A clergyman was advised to take for a cold three drops of the same preparation every five minutes for an hour. After the eighth dose he was seized with intense head-ache, which confined him to bed for forty-eight hours, and he was afterwards so weak and ill that he was unable to enter his pulpit for two months.

A young lady, aged nineteen, took for diarrhœa a teaspoonful of the same solution. She was rendered unconscious for several hours, and her nervous system was disturbed for several days.—(*British Medical Journal*, Nov. 22).

Homœopathy has now reached the culmination of its many inconsistencies, in passing “from the irrational and ludicrous extreme of



infinitesimal solutions to the dangerous extreme of the greatest possible concentration of active and poisonous drugs." Compare on this subject an interesting leader in the *British Medical Journal*, Nov. 22, exposing the scandalous frauds under cover of which many homœopaths now practise.

3. *Carbolic Acid and Zymotic Disease*.—Dr. J. Dougall has made some simple experiments on the effects of a concentrated atmosphere of carbolic acid and other volatile media on vaccine lymph, which possess considerable interest. After exposure of the lymph to the chemical agent vaccination was performed with the dried lymph, mixed with glycerin, and with the following results:—

I.—*Successful*.

Reagent								Reaction of lymph and glycerin.
Carbolic acid	-	-	-	-	-	-	-	Neutral.
Chloroform	-	-	-	-	-	-	-	Alkaline.
Camphor	-	-	-	-	-	-	-	do.
Ether	-	-	-	-	-	-	-	do.
Iodine	-	-	-	-	-	-	-	Neutral.

II.—*Unsuccessful*.

Chloride of lime	-	-	-	-	-	-	-	Acid.
Sulphurous acid	-	-	-	-	-	-	-	do.
Nitrous acid	-	-	-	-	-	-	-	do.
Glacial acetic acid	-	-	-	-	-	-	-	do.
Hydrochloric acid	-	-	-	-	-	-	-	do.

From this it appears that strong acids, or a body causing acidity by chemical affinity, *e.g.*, chlorine, are destructive of the active properties of vaccine lymph, and, therefore, *à priori* of variolous matter and other zymotic poisons. The carbolised lymph was repeatedly "vaccinated in and in," with unvarying success. These simple facts show that the present extensive use of carbolic acid as an anti-zymotic is a serious delusion.

Carbolic acid is relatively a fair antiseptic, but it by no means follows that it is *pro tanto* anti-zymotic.—(*Lancet*, August 30, 1873.)

4. *Senna, Effects of, on the Urine*.—Professor Gubler remarks that the urine of persons who have taken senna becomes of an intense yellow colour, with a green reflection, like icteric urine. If

a fragment of caustic potash be let fall to the bottom of a specimen of senna-urine, a magnificent purple colour is produced. Urine loaded with senna is incapable of assuming the variable rose-colour under nitric acid which normal urine always assumes. With rhubarb M. Gubler found a much less intense colour than with senna, and possibly in each case the phenomenon is due to chrysophanic acid, which is common to both the substances.—(*Medical Times and Gazette*, August 30, 1873.)

5. *Phosphorus, Administration of.*—Mr. A. W. Gerrard suggests the use of black resin as a solvent of phosphorus, of which it takes up at least 4 per cent. Omitting the details of preparation, the following is the formula he proposes:—Take of phosphoretted resin ( $4^{\circ}/_{10}$ ) 25 grs., powdered white sugar 75 grs., tincture of tolu a sufficient quantity. Make into 20 pills, each pill will contain  $\frac{1}{20}$  gr. of phosphorus. The advantages claimed for this formula are that it keeps well, is ready and convenient for the dispenser, and is inoffensive to the patient.—(*Pharmaceutical Journal*, December 6, 1873.)

Dr. Ashburton Thompson finds the following formula to answer all requirements:—Take of (a.) Phosphorus, 1 gr.; (b.) Absolute alcohol,  $\mathfrak{z}\text{v.}$ ; (c.) Glycerin,  $\mathfrak{z}\text{iss.}$ ; (d.) Rectified spirit,  $\mathfrak{z}\text{ij.}$ ; (e.) Spirit of peppermint,  $\mathfrak{z}\text{ij.}$  (?) Dissolve (a) in (b) by a gentle heat; warm (c) and (d) together, mix with the preceding, and add (e) on cooling.

$\mathfrak{z}\text{i.} = \frac{1}{12}$  gr. phosphorus. This forms a bright and clear mixture, almost destitute of the taste or odour of phosphorus, and stable even under exposure to light.—(*Practitioner*, July and October, 1873.)

6. *Belladonna Poisoning.*—Dr. Morel relates three interesting cases of poisoning by this drug following the use of an infusion of belladonna leaves, which had been sold, presumably by accident, for the purpose of making tea. The usual and well-known symptoms were produced, reaching in one case an extreme degree. The author makes some practical reflections upon the therapeutical means employed, and calls particular attention to the value of a watery solution of tannin as a most efficient antidote.—(Reprint from *Ann. de la Soc. de Méd. de Gand*. 1873.)

7. *Oxygen and Ozone; their Therapeutic Uses*—Dr. Técère publishes a digest in French of the conclusions which Dr. Lender, of

Berlin, has been led to by his continued researches. After pointing out the connexion between the amount of atmospheric ozone and the greater or less prevalence of epidemic diseases, it is stated that it is to the presence of ozone that we are to refer the failure of certain vaccinations on the large scale, and of some otherwise inexplicable explosions, *e.g.*, of gun-cotton and picrate of potassium. In Dr. Lender's experience the artificial introduction of oxygen and ozone into the economy repairs the strength, slows the circulation, regulates and stimulates the appetite and digestion, brings on sleep, and, in fact, acts as a general tonic. 1,000 cc. of the ozonised water contain 4.45 of ozone at 0° C. and 760 mm. One to three wineglassfuls may be taken three times a day on an average.

8. *Chloral*—(a.) *Preparation*.—In the *Journ. de Ph. et de Chim.*, October, 1873, M. Detsényi gives a good account of the manufacture of chloral as carried on at present in Berlin. In the vessel which receives the condensed hydrochloric acid disengaged in the process, M. Krämer has determined in the lower stratum an ethereal layer, composed of a mixture of chloride of ethylene and chloride of ethylidene. This latter compound has been examined by O. Liebreich, and found to possess anæsthetic properties. The exact separation of these two ethereal bodies is difficult.

(b.) *Quantitative Estimation*.—In the *Report* for August, 1871, a sketch was given of three methods for the quantitative estimation of chloral hydrate, viz., the “ammonia process” of Williams and Umney, the “sulphuric acid test” of Versmann, and the “lime process” of Wood. MM. Meyer and Haffter propose a volumetric method of simple application. From the fundamental equation,  $C_2HCl_3O + H_2O + NaHO = CHCl_3 + NaCHO_2 + H_2O$ , we see that one molecule of hydrate of chloral (*i.e.*, 165.5 grs.) saturates exactly one molecule of soda. If, then, we introduce a given weight of the chloral to be tested into a known excess of solution of soda, and afterwards determine the excess of soda by means of standard acid, the difference will be the amount of soda neutralised by the chloral, from which we can calculate the chloral. If the chloral should contain, as impurity, any free hydrochloric acid, this must previously be removed by agitation with carbonate of calcium.—(*Répert. de Pharm.*, No. 16, 1873)

(c.) *Coloration with Oil of Peppermint*.—M. Carl Jehn points out that if chloral hydrate be touched with a drop of oil of peppermint, there is soon manifested a red tinge, which gradually deepens.

The colour is not discharged by heat, and its intensity is augmented by the addition of a drop of sulphuric acid. A number of other essential oils thus tested gave no reaction.—(*Rep. de Pharm.*, No. 18, 1873.)

(d.) *Therapeutics of*.—In the July, August, and September numbers of the *Journ. de Pharm. et de Chim.*, Professor Gubler discusses, in a very interesting, and, as is his wont, a careful manner, the true position of chloral in therapeutics. At the outset he declares his dissent from the chloroform theory of Liebreich, and adduces overwhelming arguments against the supposed transformation of chloral into chloroform within the organism. So far back as 1870, *i.e.*, a year after its introduction, he publicly taught the doctrine of the *autonomy of chloral*, and it almost seems as if the simplicity of Liebreich's theory blinded the eyes of its supporters to patent, but contradictory, facts. Not to speak of the obstacle which the albumen of the serum puts in the way of the realisation of chemical phenomena indicated by theory, we know that a *strong* solution of alkali is requisite for the conversion of chloral into chloroform. Yet the blood is only a feebly alkaline fluid; moreover, in the blood of chloralised animals but a very minute quantity of chloroform has ever been detected—out of all proportion to the dose of chloral administered. M. Gubler himself has never succeeded in developing chloroform by putting chloral in contact with blood, saliva, or Vichy water, and he seems to be unacquainted with the decisive experiments of Dr. A. Gamgee on this point, which were quoted in the *Report*, August, 1870. If any of the chloral introduced into the system be changed into chloroform, it must be a very small portion indeed. Again, clinical and physiological observation exhibits fundamental differences between the action of chloral and chloroform, and, among others, the suddenness of the hypnotic influence of chloral is one of the best arguments in support of its autonomy. The hypothesis of the peculiar effects of chloral being due to the evolution of *nascent* chloroform, is both gratuitous and unnecessary.

To sum up, while chloroform is the most powerful of anæsthetics, and a hypnotic of little value, chloral, on the contrary, enjoys a hypnotic power comparable to that of opium, and only causes loss of sensibility at the moment that it threatens life, chiefly by a paralyzing influence on the heart. According to Labbé and Gubler, this paralyzing action is mainly, if not exclusively, transmitted through the medulla oblongata. M. Gubler points out that most of



the fatal accidents which have occurred under the use of chloral were in association with lesions of the heart, and he thinks that prudence should be exercised in administering chloral to patients affected with disturbances of cardiac innervation, characterised by feebleness of contraction, and palpitation or intermittency. In previous *Reports* numerous illustrations were furnished of the capabilities of chloral for doing harm as well as good, and a not inconsiderable number of deaths must undoubtedly be attributed to it. The accidents to which chloral has given rise may collectively be termed *chloralism*, and we should distinguish an acute and a chronic chloralism. The former may be slight or grave, and even fatal.

(a.) In the slight form the phenomena observed are—vomiting, especially if the patient be standing, vertigo, dulness, and loss of strength. Consecutively, various eruptions have been noticed—erythema, scarlatiniform rash, urticaria, evidences of paralysis of the sympathetic with purpura, coincident inflammations of mucous membranes, &c.

(b.) In the grave form the symptoms evinced are—paleness, disturbance of vision, cold sweats, feebleness and depression of pulse, stupor, coma, mydriasis, tonic tetanoid convulsions, and death.

In *chronic chloralism*, phenomena, reminding us of ergotism, have been met with, viz., hyperæsthesia, general malaise, desquamation of the cuticle of the fingers, with superficial ulceration around the nails, anasarca, albuminuria, enfeeblement of the heart, and oppression of respiration.

The contra-indications to chloral pointed out by Liebreich himself were enumerated in the *Report*, August, 1872. In the *Rev. de Thér. Méd. Chir.*, Juillet 15, 1873, Drs. Pelman and von Gellhorn indicate as mischievous effects from chloral—extreme narcosis, delirium, trembling, congestion of the face, complete dysuria, with spasm of the vesical sphincter, pains in the limbs, general lassitude, vaso-motor and circulatory troubles, œdema of lungs, diarrhœa, emaciation, and a marked tendency to parenchymatous hæmorrhages. Dr. W. Kirkpatrick Murphy also calls attention to the evils resulting from the wide-spread popular consumption of chloral, and he gives three interesting cases, out of many, illustrating the severe nervous symptoms which followed the too prolonged and injudicious use of chloral (*Lancet*, August 2nd and 9th, 1873). In the *Practitioner* for June, 1873, is an abstract of an important paper, by Dr. Kirn, on chronic chloral poisoning,

which, however, is too long to reproduce in this Report. Therapeutically considered, M. Gubler offers some remarks on chloral as a local alterative and stimulant, as an anæsthetic, as an antispasmodic, a hypnotic, and as a remedy in hydrophobia, epilepsy, puerperal convulsions, and tetanus. The local applications of chloral are few, and practically unimportant, and its use as a surgical anæsthetic is forbidden, because it induces loss of sensibility only at the risk of loss of life.

9. *Croton-chloral*.— $C_4H_3Cl_3O, H_2O$ . Among his "Contributions from the Pharmacological Institute of the University of Vienna," Dr. Carl v. Schroff, Jun., publishes some experiments made on frogs with croton-chloral alone, and in combination with strychnia. He first gives the details of the symptoms, resulting in death, which followed the subcutaneous injection of 0.0225 grms. of croton-chloral into a frog weighing 42.4 grms. Within fifty minutes there was total extinction of reflex excitability to any mechanical stimulation. He then gives two experiments on the combined action of croton-chloral and strychnia. In the one, 1 mgm. of strychnia was injected into a frog weighing 35.5 gm., four minutes after a dose of 0.015 gm. croton-chloral. Death ensued more rapidly than in the previous control-experiment. In seven minutes after the administration of the strychnia intense tetanus was developed, and after three hours reflex excitability was completely lost, the tetanic state having gradually diminished. After five hours the thorax was opened, and the ventricle was found to contract imperfectly.

In the other, 0.6 mgm. strychnia was administered to a frog weighing 37.5 gm., four minutes after the same dose of croton-chloral, *i.e.* 0.015 gm. In seven minutes intense and protracted tetanus set in. After fifty-seven minutes reflex excitability was nearly extinct, but after six hours it returned; during the next two days it was exalted, the slightest touch calling forth energetic contractions, and early on the fourth day the animal was found dead.—(*Stricker's Mediz. Jahrb.*, 1872, IV. Heft).

In the *British Medical Journal*, December 20, 1873, is a short paper by Dr. O. Liebreich on the action and uses of croton-chloral hydrate, which contains some interesting speculations, based on his theory of the decomposition of chloral within the body. The distinguished professor says:—

"Croton-chloral differs in its outward appearance from hydrate of

chloral, by its being dissolved with difficulty in water, and by its crystallising in small glittering tablets. Its action, though similar to that of hydrate of chloral, differs widely from the latter with regard to its physiological effects. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesia of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered.

“I have experimented with this remedy on maniacs during an attack of mania. They remained quietly sitting on their chairs in a deep sleep, their pulse and respiration being unchanged for two whole hours together. If anæsthesia had reached so high a degree in consequence of the application of hydrate of chloral, the patients would have dropped from their chairs, and both their pulse and respiration would have been considerably retarded. I have seen croton-chloral acting in the same way on healthy individuals. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it.

“The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable, on account of heart-disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral.

“Whilst examining the difference between the action of hydrate of chloral and that of croton-chloral, I have discovered the remarkable fact that it is not the first, but the second, product of decomposition of the latter substance which is brought into action, on account of the first being too rapidly destroyed. Croton-chloral, when subjected to the influence of an alkali, first forms allyl-chloroform, a trichlorated body, which is rapidly decomposed into a bichlorated substance called bichlorallylene. Now, both chloroform and trichlorated substances act, as I have shown, in their first stage on the brain, in the second on the spinal cord, and in the third on the heart. The retardation of respiration is to be explained by the agency of these substances on the last-mentioned organ. Bichlorated substances act differently, as is proved by bichloride of ethylene. Even if the circulation of the blood in an animal have been stopped by this latter agent for one minute, life may be restored by artificial respiration, which is impossible whenever trichlorated substances have produced

this effect, in which case the muscles of the heart remain paralysed. Well, in animals poisoned by croton-chloral to such a degree that both circulation and respiration are stopped entirely, artificial respiration is able to restore the action of the heart immediately, and the life of the animal may thus be saved. Bichlorallylene, inhaled by the lungs, produces the same effects on animals as croton-chloral. We thus see these bichlorated substances acting on the brain, spinal cord, and medulla oblongata, but not on the heart, which explains the fact that both respiration and circulation remain unaltered in man by a medicinal dose. It is a highly interesting fact, however, that under favourable conditions we still are able to produce in animals the effects of the first product of decomposition of croton-chloral—i.e., of the trichlorated substance or of allyl-chloroform. In order to observe these effects it is necessary to introduce immense doses of croton-chloral into the body, when paralysis of the heart actually does ensue.”

Dr. Wickham Legg's favourable experience of croton-chloral in the treatment of paroxysmal pain of the fifth nerve was noticed in the last *Report*, August, 1873, and more recently Dr. Benson Baker speaks of it as being of the greatest service in cases of nerve pain, given in doses of 1 or 2 grs. every hour or two. In thirteen patients not a single bad symptom was observed. It relieved pain quickly, caused natural sleep, and in several instances acted as a gentle laxative, and evoked no subsequent head-ache or digestive disturbance.—(*Brit. Med. Journ.*, October 25, 1873.)

10. *Nitrate of Lead in Onychia Maligna.*—Mr. W. MacCormac, in the course of some remarks on this painful and intractable disease, adduces the experience of one of his pupils, Dr. J. Scott, of Belfast, who confirms Moerlose's and Vanzetti's statements as to the marvellous effects of this lead salt on the unhealthy ulceration of onychia. Dr. Scott asserts that the local application of powdered nitrate of lead may be relied on implicitly; no cutting of the nail or matrix is required, and a complete cure is effected in from fourteen to thirty days.—(*Brit. Med. Journ.*, December 6, 1873.)

Some notice of Dr. De Moerlose's experience was given in the *Report*, August, 1869.

11. *Nitrite of Amyl in Epilepsy.*—Dr. Crichton Browne (*West Riding Lun. Asyl. Med. Rep.*), in the course of his investigations on the pathology of blushing, was struck by the fact, that the degree and extent to which the blushing caused by nitrite of



amyl is manifested are influenced by certain pathological states. He found that general paralytic patients may inhale a considerable amount without displaying any marked flushing, even of the face, and that epileptics cannot breathe the smallest quantity without exhibiting extreme cutaneous hyperæmia over the face, neck, and chest. Reasoning from this, he was led to conclude that if the nitrite could be given immediately before an epileptic fit, the spasm of vessels might be prevented, and so the whole sequence of morbid events averted—a gain of no slight importance. In several cases in which the nitrite was administered immediately after an aura the usual fit did not supervene, and in one case in which it was administered regularly three times a day a series of fits, from which the patient was suffering, was abruptly interrupted. In rabbits, too, rendered artificially epileptic by Professor Ferrier, it was noted that the fit which invariably followed on electrical irritation applied to the exposed brain, when no interference took place, was arrested by the inhalation of nitrite of amyl.

Dr. Browne thinks that this agent will supersede other methods of attempting to avert the fit by acting upon indications afforded by the aura, and a vinaigrette, or small stoppered bottle, containing a sponge soaked in nitrite of amyl, and carried in the pocket, will probably be found a safeguard to many sufferers from epilepsy. But even in the fully developed stage of epilepsy the nitrite may be beneficial, and in ten cases of the *status epilepticus* in which it has been used eight have terminated in recovery. Under its influence several patients have rallied from what was apparently a hopeless condition; the breathing becomes freer, the circulation is relieved, and it appears to act with a directness and certainty that cannot be ascribed to any other remedy hitherto employed in the *status epilepticus*.—(*Med. and Surg. Rep.*, October 25, 1873, from *Med. Press and Circ.*)

12. *Alkaloids*.—(a.) *Reaction with Sugar and Sulphuric Acid*.—Schneider having observed the following test for morphia, extended his inquiries to other alkaloids.

When a drop of sulphuric acid is added to an intimate mixture of one part of *morphia* with six or eight parts of sugar, a very intense purple colour is produced, which, after half an hour, as it absorbs moisture, passes to violet, then to a blue-green, and finally to a dirty yellow. This reaction is one of great delicacy. To use it for the detection of morphia in a solution, a single drop should be

taken, and sugar added to it to saturation; a drop of sulphuric acid is then placed by the side of it, and when the two drops meet, the coloration takes place.

With *codeia* also a purple is produced, which passes to violet, and then to yellow-brown. *Narcotia* and *narceia*, under the same conditions merely produce brownish colorations. *Quinia* and its salts produce a brown colour. In the presence of water no coloration is produced, but there is simply an increase in the quinine fluorescence. *Strychnia*, *brucia*, *atropia*, *colchicia*, and *picrotoxia*, give no characteristic reaction. *Aconitia* gives rise to a rose-coloured zone, which passes rapidly to violet, then to brown. It might thus be confounded with *codeia* and *morphia*, but it differs from the latter in that chloroform or benzine removes it from an alkaline solution, and from *codeia* by its solubility in boiling water. *Delphinia* gives a spot of yellow-brown, surrounded by a dirty green zone. Upon the addition of water the brown passes to green. This is a characteristic reaction.—(*Pharm. Journ.*, October 11, 1873, from *Journ. de Ph. et de Ch.* (4.), xvii., from *Bull. Soc. Chim.*) In connexion with the *morphia* reaction, a new test, proposed by Mr. Siebold, should be noticed. He states that 0.0001 grm. of *morphia* can be distinctly recognized by the deep brown colour developed, on heating it gently with a little pure sulphuric acid and a very small quantity of pure perchlorate of potassium. No other alkaloid is acted upon in a similar way.—(*Pharm. Journ.*, October 18, 1873) Kalbrunner states that he can detect one part of *morphia* in 14,000 of solution by the deep blue colour developed with Kieffer's test, viz., ferric chloride and ferridcyanide of potassium.—(*Rép. de Pharm.*, No. 19, 1873.)

(b.) *Quantitative Estimation*.—Mayer employs a standard solution, of the iodohydrargyrate of potassium, prepared by adding corrosive sublimate in excess to iodide of potassium, to determine quantitatively all the vegetable alkaloids, whether pure, or as contained in pharmaceutic preparations.

The precipitate of the double iodide of mercury and alkaloid undergoes no alteration by concentrating the liquid. None of the ordinary excipients of pharmaceutical preparations hinder the reaction, excepting alcohol and acetic acid, in either of which the precipitates are soluble. This process, of course, is applicable only to solutions containing, or supposed to contain, a single alkaloid. Mixtures of alkaloids must be separated by the ordinary methods.—(*Journ. de Ph. et de Chim.*, October, 1873.) Zinoffsky also employs

the same method in the estimation of the respective alkaloids of ipecacuanha, aconite, and tobacco. The volumetric mercurial solution contains 13·546 grms. of corrosive sublimate, and 49·8 grms. of iodide of potassium in one litre of water. One cc. of this solution precipitates  $\frac{1}{10000}$  or  $\frac{1}{20000}$  of an equivalent of alkaloid.—(*Pharm. Journ.*, December 6, from *Bull. de la Soc. Chim.*)

(c.) *Aconite Alkaloids*.—The “battle of the aconitines” has been going on for at least twenty years, and much credit is due to Mr. T. B. Groves for his continued and successful efforts to unravel this intricate and disputed question. In 1870 Mr. Groves exhibited *pseudaconitine* at the Liverpool meeting of the British Pharmaceutical Conference, and last year he set to work on fifty pounds of Nepaul aconite root, and succeeded in obtaining no less than  $1\frac{1}{2}$  ozs. of *pseudaconitine*. He has also experimented on the characteristics of the various aconitines met with in commerce, and has made out many points of interest. Mr. Groves thinks it probable that there are in truth two series of allied alkaloids—the one furnished by *aconitum napellus*, the other by *aconitum ferox*, or some other Indian aconite. Each species seems to furnish its *crystallized aconitine*, its *amorphous aconitine*, and its *napellin*. Should this be correct it is obvious that the term special napellin should be abandoned. He asks, too, why should the alkaloids of the Indian aconite be stigmatised as *pseud*, or false? The prefix *Ind.* would be better, or, if the name napellin be abandoned, *nepallin* might, without danger of confusion, be applied to *pseudaconitine*. He is disposed to think that Morson’s aconitine is *pseudaconitine*, with its apparent solubility increased by a large admixture of the so-called napellin. The colour reactions of these alkaloids may be dismissed in a word—there are *none*.—(*Pharm. Journ.*, October 11, 1873.) Further details on crystallized aconitine and on *pseudaconitine* will be found in the *Reports* for February, 1871, and August, 1872.

(d.) *Toxic Effects of Ammonium-bases*.—In the course of some uncompleted researches upon the amines, or compound ammonias, M. Rabuteau found that whilst the most diverse salts of the primary, secondary, and tertiary amines, from the methyl-amines to aniline and the phenylamines, are comparable in their physiological effects to other ammoniacal compounds, and would be, according to the author’s experiments, muscular poisons in large doses, the salts in which all the hydrogen of the ammonium is replaced by alcohol radicals, such as the iodides of tetramethylammonium and tetramylammonium, appear to be poisons of the nerves of movement

completely analogous in their effects to those of curara. The most striking point in these results is that the substitution of  $C H_3$  for all the hydrogen of ammonium, transforms a slightly active compound into an extremely poisonous one, operating similarly to curara. However, it should be remembered that the compounds obtained by treating the various alkaloids with the hydriodic ethers all produce analogous effects; thus, iodide of methyl-strychnium likewise acts by paralysing the motor nerves.—(*Pharm. Journ.*, May 31, 1873, from *Comptes Rendus*, lxxvi., p. 887.)

(e.) *The Cinchona Alkaloids*.—It is curious to observe how pre-dominantly quinine has absorbed attention as a febrifuge agent to the almost total exclusion of the other definite bases which are known to exist in the cinchona barks. Relative to quinine the prevailing impression seems to be that the other alkaloids are comparatively worthless, and, at present, cinchonidine can be obtained at less than half the price of quinine. This is the more remarkable if it be true, as Mr. J. E. Howard thinks, that the early reputation of cinchona bark was gained by a kind in which cinchonidine is the prevailing alkaloid, as was illustrated in some recent analyses of Java barks. Mr. Howard states that, of the less known cinchona alkaloids, cinchonidine has been most neglected, in great measure owing to the confusion, scientific as well as commercial, which arose between quinidine and cinchonidine—the name quinidine being still applied by German writers to cinchonidine. The sulphate of quinidine of commerce has hitherto consisted of a mixture of the two alkaloids, and in some cases of almost pure sulphate of cinchonidine. The Madras and Bombay Commissions, appointed in 1866, for the purpose of testing, on a large scale, the efficacy of the sulphates of cinchonidine, quinidine, and cinchonine, reported that all these alkaloids are febrifuge, anti-periodic, and tonic, and similar in their general effects to quinine. They are about one-half or two-thirds the strength of quinine. In 1866, Dr. J. Dougall, after some years experience in the usual treatment of malarious fevers in tropical countries, was induced to experiment on the comparative utility of these alkaloids. A series of 108 cases of intermittent fever in India, chiefly of the quotidian type, were treated in nearly equal proportions with quinidine, cinchonidine, and cinchonine. All of these cases recovered; and in three cases only was it necessary to prolong the treatment to twelve or thirteen days. Dr. Dougall states that there was very little difference between the alkaloids in the necessary duration of



the treatment, but a difference was observed in the doses required. The conclusion drawn was, that quinidine is the most powerful, cinchonidine next to it, and cinchonine the least active; but even cinchonine is energetic, and in an adequate dose is a sure remedy. The relation of these alkaloids to quinine, in point of energy, was not made the subject of direct trial; but previous experience leads Dr. Dougall to believe that quinine is equally active and certain in doses between two-thirds and three-fourths of those of quinidine. These inquiries are of great practical consequence, since it has been established by frequent recent experience that, under various circumstances, quinidine, cinchonine, and cinchonidine, are largely obtainable from several valuable cinchona barks used by manufacturers for obtaining quinine, and that they are by no means the feeble, much less the inert, febrifuge remedies which at one time they were supposed to be.—(*Pharm Journ.*, Nov. 15, 1873.)

PART IV.  
MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE  
COLLEGE OF PHYSICIANS.

*Wednesday, January 14th, 1874.*

JAMES FOULIS DUNCAN, M.D., President, in the Chair.

*Climate and Vital Statistics of Tasmania.* By J. W. MOORE, M.D., Dubl.;  
Dipl. State Med., Trin. Coll., Dubl.; F.K.Q.C.P.I.

CLIMATOLOGY is a branch of medical science which has as yet received but scant attention at the hands of our Profession. This was, perhaps, not to be wondered at, so long as meteorology remained an occult science, and the modern facilities for travel were unknown.

But now that telegraphy has established the study of the weather on a sure basis, while steam has conquered distance both by land and sea, persistent neglect of a most important subject—such as that of climatology undoubtedly is—is as indefensible as it is unwise. To many amongst us questions like the following are put daily—"Where would you recommend me to winter?" "What sea-voyage should I take?" "Is not the *mistral* severe at Nice?" and so on. To these we must be ready to give an answer, or the faith of our patients may be sorely shaken.

Considerations like the foregoing led me to think that a paper on the Vital Statistics and Climate of Tasmania would not be unacceptable to the Medical Society of the College of Physicians, especially as the subject matter of the paper, in its relation to State Medicine, clearly comes within the scope of this truly representative meeting of the Profession. I claim no originality for the remarks which follow. They have been suggested by the perusal of a "Blue Book" relating to Tasmania, which fell into my hands some months ago. This book forms part of the Proceedings of the Royal Society of Tasmania for 1872, and contains the results of thirty years' meteorological observations made at Hobart Town—principally by Mr. Francis Abbott, F.R.A.S., together

with a most interesting paper on the Climate and Vital Statistics of Tasmania, by Dr. E. Swarbreck Hall.

In studying the vital statistics of a given place or country, two factors of paramount importance engage our attention—(1), the birth-rate ; (2), the death-rate.

1. The birth-rate of a locality is a direct indication of material prosperity, or the reverse. But it may be far more than this ; for a high birth-rate *in the presence of a low death-rate* implies that a population is living under the most favourable conditions possible as regards health, vigour, and longevity. *Health*, because a larger proportion of the population survive to a marriageable age—*vigour*, because more individuals are fitted to propagate their species—*longevity*, because the more numerous the births are to a marriage, the greater the presumption that a long interval exists between the mean period of marriage and the mean period of death.

Dublin, I regret to say, does not show a favourable birth-rate, as will appear from a reference to the following Table, with which Dr. Stokes illustrated his lecture on “Sanitary Science,” last season, at the Royal Dublin Society :—

TABLE showing the Births and Deaths per 1000 of the Population living, in Edinburgh, Glasgow, London, and Dublin, in the eight years 1865–72 inclusive, with the number of persons to an acre in 1868 and 1872.

Years	Edinburgh		Glasgow		London		Dublin	
	Births	Deaths	Births	Deaths	Births	Deaths	Births	Deaths
1865	36	28	42	33	35	24	28	26
1866	36	27	42	30	35	26	28	29
1867	36	27	42	28	36	23	26	27
1868	38	27	42	31	37	24	28	25
1869	38	30	40	34	35	25	26	24
1870	38	26	41	30	35	24	27	25
1871	34	27	39	33	34	25	29	26
1872	32	26	41	28	35	21	27	29
Mean 8 years	36.00	27.25	41.12	30.88	35.25	24.00	27.38	26.38
Years	Persons to an Acre		Persons to an Acre		Persons to an Acre		Persons to an Acre	
1868	40.0		88.9		40.1		32.8	
1872	47.1		98.5		43.0		31.3	

### Ratios of Deaths to Births.

				Per cent.
Edinburgh	-	-	-	75.7
Glasgow	-	-	-	75.1
London	-	-	-	68.1
Dublin	-	-	-	96.4

### Ratios of Births to Deaths.

				Per cent.
Edinburgh	-	-	-	132.1
Glasgow	-	-	-	133.2
London	-	-	-	146.9
Dublin	-	-	-	103.8

2. The death-rate of a place is a study of surpassing interest, whether we regard its influence for good or for evil on the fortunes and future of the community, or consider it from many other points of view. On this occasion I would merely allude to (*a*) the annual general death-rate, (*β*) the death-rate of children under 5 years of age, and (*γ*) the zymotic death-rate, in connexion with the more immediate subject of this paper.

(*a*.) The general death-rate, or that from all causes, teaches us a great deal—by it we are enabled to compare the mortality of one place with that of another, or of the same place from year to year. But we require more than this.

(*β*.) The death-rate of young children affords, in addition, an accurate gauge of the health standard of a population. Where infant mortality is high, the sanitary condition and local relations under which a population live are sadly out of gear, the *physique* of the people is of a low type; and probably climatic influences are to blame.

(*γ*.) Lastly, the zymotic death-rate, if high, tells of the invasion of epidemics, of the existence of endemic maladies, of the prevalence of baneful ignorance, want, overcrowding, dirt, and disease.

From all this we gather that two factors largely influence the death-rate—(1.) Climate, (2.) Defective Sanitary Conditions. On these I need scarcely dwell on the present occasion. The influence of the first will be recognized by any one who has traced the rise of the mortality from thoracic affections at the approach of winter in our own country, and the equally well-marked increase in the deaths from diarrhœa, enteric fever, *et hoc genus omne*, as the summer rolls by. The part played by the second will be as readily conceded by those who have studied the history of the black death of the middle ages—of cholera in its travels from India to Russia and to the Mediterranean—and of the outbreaks of enteric fever which have been investigated of late years in this and the sister country. All I wish to do here is to lay down these points, because of their important bearing on the present paper.

*Climate of Tasmania.*—Lying between the parallels of 40°40'–43°40' S. lat., and between 144°30'–148°30' W. long., separated from the conti-



nent of Australia by about 200 miles of water (Bass's Strait), and washed on its western, southern, and eastern seaboard by the South Pacific Ocean, the Island of Tasmania (or Van Dieman's Land) possesses a typically temperate and insular climate. It is just five-sixths the size of Ireland; the scenery is beautiful—wild high-lands, alternating with open plains, gentle undulations covered with forests, or deep and lovely valleys, through which flow numerous rivers from the central lakes cradled in the midst of lofty mountains. Many of these exceed 4,000 feet, and Ben Lomond and Cradle Mount are both above 5,000 feet in height.

I have been particular in giving the latitude of the island, because a moment's reflection will show that it alone would tend to produce a temperate or warm climate—it is the latitude of southern and central Italy.

We shall now briefly consider the meteorological data, in order, under the headings—(1) Temperature; (2) Direction of the wind; (3) Barometrical pressure; (4) Humidity of the air; and (5) Rainfall. The observations, as I have said, were made at Hobart Town, nearly 43° S. lat.

1. *Temperature.*—The average mean temperature of 30 years was 54·72° F., just 5° higher than the average annual mean temperature of Dublin. The mean of the warmest month (January) was 62·69°, or 1·5° higher than the average mean of July here, and the mean of the coldest month (July) was 46·07°, or nearly 7° higher than the average mean of our January. It will be observed that while the Tasmanian summer is scarcely, if at all, warmer than the corresponding season in England (mean temperatures of the three summer months in the two countries—62° and 61° respectively)—the Tasmanian winter is fully 8° warmer than the English winter (47° and 39° respectively). Frost of any intensity is rarely experienced at Hobart Town, and snow does not lie on the low-lands. Of course on the mountains and central table-lands the winter is sufficiently rigorous. In summer, occasionally, intense heat is felt when a north wind blows over the island from the parched plains of Australia. Under these circumstances the thermometer may rise to 100° in the shade, but such an occurrence is the exception, not the rule.

2. *Direction of the Wind.*—When we study the prevailing winds the mildness of this wonderful climate is largely accounted for. During the four coldest months (May–August) a north-westerly wind blows almost without interruption. It corresponds to our south-westerly wind, and, like it, is an equatorial and an oceanic current. This north-westerly wind is, in fact, the “counter-trade,” or upper current which descends to the earth's surface on the polar edge of the trade-winds. It is moist, but not excessively so. In spring there is a conflict between the north-westerly and south easterly or trade-wind, and, as summer approaches,

the sun moving southward, Tasmania is more fully brought within the influence of the latter current. We thus have the summer heat tempered by cool polar and oceanic breezes. In autumn the S.E. dies away, and the N.W. becomes again predominant. The force of the wind is least in May and July, and greatest in November; but the climate is by no means a stormy one.

3.—*Barometer*.—The average annual mean pressure was 29·850 inches—almost identical with the English height of the barometer. The mean is given by Mr. Abbott as highest in November (30·065 inches), but there is internal evidence of some mistake in relation to this point. The mean is lowest in those months when the opposing air-currents, N. W. and S. E., are in conflict; that is, when cyclonic storms are likely to be more common.

4.—*Humidity of the Air*.—As might be anticipated, the Tasmanian climate is drier than ours. The per-centage of humidity varies from 83 in June to 67 in January and December, the mean annual amount being 75. The dew point varies from 50·51° in February to 40·03° in July, so that even in the coldest month it is 6° below the temperature of the air.

5.—*Rainfall*.—The average annual rainfall at Hobart Town was 22·71 inches. At Dublin, for the 7 years (1865-71 inclusive), it was 25·63 inches, and in 1872 [that *annus mirabilis* of rain] 35·57 inches were registered by me. The quantity of rain in Western Tasmania, as in Western Ireland, is much greater; in some districts it is as much as 75 inches. Spring is the season in which most rain falls, corresponding to the period of barometrical and wind disturbances. The average annual number of rainy days was 140, of which only 25 fell within the 90 days of January, February, and March. According to my observations, the average number of rainy days in Dublin, in 7 years, was 179; in 1872 rain fell on 238 days!

To sum up—we see then that in Tasmania all the conditions of climate are favourable to life—a mean annual range of only 17° F., an open winter, a temperate summer, ozone-laden sea breezes at all seasons of the year, clear skies, a moderate and evenly distributed rainfall, and an absence of damp and fog in winter—all combine to realize the *beau idéal* of a healthy and charming climate.

We can now understand Mühry's statement, as given by H. Rey in his splendid article "Géographie Médicale."<sup>a</sup>

"Le pays jouit d'une parfaite salubrité; il n'y a aucune maladie

<sup>a</sup> Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques. Tome seizième, p. 357.

endémique dominante. On n'y voit pas de *fièvre de malaria*. Le défrichement des terres neuves, parfois si dangereux pour les premiers colons, est ici sans péril."

Scoresby-Jackson also, in his valuable work on "Medical Climatology" (page 159), speaks of the salubrity of the climate, "which is," he says, "highly esteemed, and frequently resorted to by invalids from India."

*Vital Statistics of Tasmania.*—Census enumerations of the population were taken in 1857, 1861, and 1870. In 1857 the total population of the island was 81,492; in 1861, 89,977; and in 1870, 99,328. The estimated population on the 30th June, 1871, was 101,869. It may be well to mention that by this time the aborigines of Tasmania are probably extinct; at least, in 1865 there were only 6 individuals of the race remaining, and 2 years later this small number had been still further reduced to 3. The present population is almost exclusively of European descent. In 1870, 6,600 persons above 60 years of age were alive, or 6·5 per cent. of the whole population, and 46 persons were aged 90 or upwards.

*Birth-rate.*—In the 16 years ending and including 1871 the average annual number of births was 3,063, in the ratio of about 105½ boys to 100 girls. The birth-rate for the census years was, respectively, nearly 40 per 1,000 in 1857, about 35 per 1,000 in 1861, and only about 30 per 1,000 in 1870, the average rate for 16 years being about 34 per 1,000. The falling off of late years is remarkable, but its explanation is simply that adult males have flocked to the newly-discovered gold diggings in adjacent colonies, and so both the marriage and the birth-rates have diminished temporarily. Of 99,328 persons living in 1870, 59,119 had been born in Tasmania.

*Death-rate.*—Owing to the influence of the exceptional and transitory factor just mentioned on the birth-rate, we are compelled to depend, even more than we otherwise should have done, on an investigation of the death-rate, in arriving at an estimate of the health standard of Tasmania. The deaths registered in the whole island, in the 15 years ending and including 1871, numbered 21,395, equal to a yearly average of 1,426·3. The average for the first 7 years was 1,485, while that of the last 7 years fell to 1,366·6, and this notwithstanding the increase of population. In 1857 the death-rate was 17·5 per 1,000 living; in 1861 it fell to 16·25; in 1869 to 13·5; in 1870 it was 14; and in 1871 it again fell to 13·25 per 1,000. The lowest general death-rate (about 13 per 1,000) occurred in 1865, when only 1,263 deaths were registered. Speaking of this, Dr. Swarbreck Hall writes as follows:—

"The weather, generally, in 1865, was of the most favourable

character. \* \* \* Two causes, principally, seem to have operated in producing the annually decreasing rate of mortality in Tasmania. The first being the constantly increasing proportion that those born in the Colony bear to the imported inhabitants; at the same time, by the ordinary laws of mortality, a disproportionately large share of children adds to the death-rate. The second seems to have arisen from climatic improvements, the meteorological phenomena of late years having certainly been more auspicious to health and life—though, as these changes are usually cyclical in their character, we must expect a return to the old type for another period of years, ere long. Sanitary improvements, moreover, and specially the enlarged and much purer water-supply in both the southern and northern capitals, has doubtless improved the health and reduced the deaths of their citizens. Were as valuable a change effected in the sewerage of the two towns, in the ventilation of our houses, in the abolition of intramural interments, in more watchfulness over the purity of our food supplies, and last, though by no means the least, less intemperance in drink, the mortality rate might be reduced to an amount which the most sanguine of sanitarians have never yet ventured to predict, and which no country in the world has so far exhibited. If we will only take all the advantages science affords to utilize and improve the blessings the Giver of all good has conferred upon our beautiful Island, there will indeed be a happy future for little Tasmania amongst the great nations of the southern hemisphere.”

Truly, the Tasmanian death-rate is marvellously low. Dr. Hall compares it with that of England in a paragraph which I will quote:—

“The death-rate for all ages in England and Wales averages about 22 per 1,000 per annum. The death-rate of Tasmania, on the average of the last three years, 1869, 1870, 1871, is  $13\frac{1}{2}$  per annum, or less than the English by  $8\frac{1}{2}$  per 1,000. The healthiest registration districts in England and Wales have 14 to 15 per 1,000 per annum. Dr. Wm. Farr, the eminent head of the vital branch of the Registrar-General’s Department in England, now President of the Statistical Society of London, considers that 17 per 1,000 per annum may be assumed to be a normal death-rate where the population is fairly proportioned according to sex and age. The data I have now adduced prove that the Tasmanian death-rate is far less than the normal standard, even with a population having so much greater a proportion of the very young and the very old.”

The last remark leads me to consider the Death-rate of Young Children. In the 15 years we have been discussing in this paper the average yearly number of deaths among children under 5 years of age in Tasmania was 489·73; in other words, 34·3 per cent. of all the deaths were of children under 5. At first sight this does not appear a favourable result. In



Dublin, during the 9 years, 1864–72, only 30·3 per cent. of all the deaths were of children under 5, and, as we all know, the mortality of children in this city is unhappily very great.

But it is evident that the ratio of the per-centage of the death-rate at any age to the general death-rate will be influenced most materially by the *proportion existing between the number of persons at the given age living in a community and the total population*. When due allowance is made for this we find the following results:—

In Tasmania, in 1870, the number of children under 5 years' old was 14,302. Of these, 409 died within the year. This gives a mortality *in this class of the community* of 29 per 1,000.

In Dublin Registration District, in 1869 (the healthiest year since 1864), the number of children under 5 years of age was (approximately) 34,020. Of these, 2,320 died within the year; equal to a mortality *in this class* of 68·2 per 1,000, considerably more than twice the death-rate in Tasmania. In 1872 (an unhealthy year, owing to small-pox) the rate in Dublin among children under 5 rose to 74·9 per 1,000—nearly *three times* that in Tasmania. It may be objected, that this comparison between a city and a sparsely populated country is not fair. To meet this, I have merely to say that the Hobart Town returns are almost as favourable as those for all Tasmania; that the total number of deaths in the capital, in 1870, was 500, including 123, or 24·6 per cent. of children under 5 years. The population living at this age was 2,594, so that the death-rate was 47·5 per 1,000, against 68·2 per 1,000 in Dublin. Dr. Hall also points out that the death-rate of children under 5 in England and Wales is 67·5 per 1,000, and in the 63 healthiest districts of that country 40 per 1,000. He also shows that the average death-rate of children under 1 year has been in Tasmania 100·4 per 1,000; in England, 165·5. In 1871 it fell to 87·5 per 1,000 in Tasmania.

I now pass to the Zymotic Death-rate. The available statistics relate to four years only—1868, 1869, 1870, and 1871. The average total number of deaths in each of these years was 1,389·5. Of these, 205·75 deaths were caused by zymotic diseases. That is to say, *less than one-sixth* of the total mortality was due to this class. In England the proportion is nearly *one-fourth*, and in Dublin (on an average of 8 years, 1865–72), it has been 24·1 per cent. In Hobart Town, in 1870, 52, out of 500 deaths, were attributed to zymotic diseases. This gives a per centage of only 10·4; the lowest in Dublin being 17·6 in 1864, a year of defective registration.

Looking more closely into this subject, we find that *Small-pox* is as yet unknown in Tasmania, a circumstance which has, unfortunately, caused a general and deplorable neglect of vaccination, only 25 individuals having been vaccinated, in 1868, in the three institutions in the country where public provision is made for vaccination, namely, the

hospitals at Hobart Town, Launceston, and Campbell Town. *Measles* was fatal in only 2, and *Scarlatina* in only 10 instances during the 4 years. Both diseases have, however, often assumed an epidemic form; notably, scarlatina in 1853, when it prevailed with great fatality, and measles in 1861 and 1867. In the last-named year a severe epidemic of measles prevailed in Dublin also. *Diphtheria* has been endemic since 1859, and caused 45 deaths in the 4 years. *Croup*, which Dr. Hall has always "traced to sudden changes of temperature," caused 69 deaths. It is especially prevalent in Hobart Town, and almost unknown in the inland districts. *Whooping-cough* prevailed in 1868 and 1869, causing 119 deaths. *Typhus*, *Infantile*, and *Typhoid Fevers* proved fatal in only 99 instances in the 4 years, the mortality being a little more than *one-fourth* of that from these diseases in England. Dr. Hall attributes the "antifebrile" nature of the climate to the abundance of ozone in its prevalent sea-breezes. *Dysentery* caused 72, and *Diarrhœa* 251 deaths. The former disease, although it is as yet more fatal than in England, is rapidly diminishing. The latter is the fifth most fatal disease in Tasmania, prevailing especially in the summer months of December, January, February, and March, and sometimes assuming the proportions of an epidemic. It is, however, not so fatal by nearly one-third as it is in England, and in Hobart Town and Launceston a marked and permanent diminution in its fatality has been noticed since the introduction some years ago of an improved water supply. *Asiatic Cholera* has never appeared in Tasmania. *Ague* is not endemic—2 imported cases proved fatal in the 4 years. *Rheumatism* caused 39 deaths, but is not as fatal as in England. Among enthetic affections, *Syphilis* shows a death-rate only *one-fifth* that in England.

Only a few words as to *Constitutional* affections. Among the diathetic order we observe that *Gout* is very rare, and scarcely ever fatal in Tasmania, while *Cancer* is more fatal there than in England. Among the tubercular order we learn with surprise that *Scrofula* caused only 13 deaths in the four years, not one-fourth of the English rate. On this, Dr. Hall remarks—"With the pure, dry air, bright skies, moderate temperature, and comparatively uncrowded habitations in Tasmania, all classes obtain abundance of animal and other food, and live much in the open air."

*Phthisis* had 375 deaths, but little over one-third of the English mortality from this affection. It is the most fatal disease in England, it stands but third on the list in Tasmania—old age and heart disease taking precedence. Dr. Hall writes as follows:—

"Of the preservative and curative effects of the Tasmanian climate to those having the consumptive diathesis, I could, from my personal experience, give many illustrations; but one striking instance may

suffice. I know a lady, now in her sixty-fifth year, who, when twenty-six years old, left England with her husband for this Colony, not being expected either by her husband or father, both medical practitioners, to survive the voyage; she had lost all her offspring either by miscarriage or premature births. Since her residence in this Island she has never had a consumptive symptom, and has a grown up family all free from any sign of the tubercular diathesis. She was the oldest of three sisters—the other two, many years ago, succumbed in their native land to this dire scourge of England's daughters."

Time would fail me to go through the remaining classes of disease. But I must allude to the mortality from *Bronchitis*, that disease which, more than any other, tends to raise the death-rate of the winter months in our foggy, cold, and wet climate. In Tasmania it caused 214 deaths in the four years, the annual rate per 100,000 inhabitants being 53·5. It ranks but seventh in the order of fatality. In England the annual rate is 134·5, and it is the second most fatal disease. The returns of *Pneumonia* are almost as favourable, the annual death-rates being in Tasmania 61·25, in England 124, per 100,000.

Dr. Hall writes:—

"Longevity in Tasmania, on the total population both of native-born and imported inhabitants, judging from the death-rates, seems to be very great. The Island, of course, has not been colonized long enough to furnish data for an exact calculation of longevity in the native-born alone, as there are as yet but few of them above sixty years old."

He then compares the death-rate of those above sixty in the Isle of Wight with that in Tasmania, and proceeds:—

"It would thus appear that the chances of long life in Tasmania even for the imported inhabitants, is much greater than in England—though a large proportion of those over sixty years old have been a depraved class from the British Isles, and hard livers in this Colony. The oldest death ever recorded in Tasmania was of a man at one hundred and eight years of age, but there have been several above one hundred years, *i.e.*, 13 from 1851 to 1871 inclusive. Only a few weeks ago a man aged one hundred died in the Hobarton Hospital, and he had lived sixty-seven years in these colonies. His death, too, was caused by a fall fracturing the upper part of the thigh bone, and not from natural decay or disease. He was, moreover, both active and intelligent previous to the accident. A woman of the well-ascertained age of one hundred and five years died under my care when I had charge of the female invalids about eighteen years ago. Another, aged one hundred and four, I knew to be able to ride about the country on horseback, Peruvian fashion, when she was one

hundred years old. The death in the table, in 1869, was a man aged one hundred and three years and nine months old."

In conclusion, I would sum up the points which principally establish the salubrity of this southern climate :—

- (1.) The very low general death-rate, which is—so far as I am aware—without a parallel.
- (2.) The low death-rate of children, and
- (3.) The tendency to longevity—both accounted for in a great measure by the mild winters and temperate summers.
- (4.) The comparatively low zymotic death-rate.
- (5.) The lessened mortality from bronchitis, pneumonia, and other pulmonary affections—due almost exclusively to the open winter season.

The PRESIDENT observed that Dr. Moore's paper was interesting and valuable. All the calculations, however, which he had entered into required very close investigation. Many fallacies might creep in unawares, and, unless these were sifted carefully, false conclusions might be drawn—more particularly the comparison between an old and a new country might be erroneous and misleading.

DR. GRIMSHAW thought the Society should feel obliged to Dr. Moore for bringing forward a paper of this sort, which was almost one of the first of the kind they had had before them. Such papers were important to those who had to select places of residence for invalids. Only last week he was asked whether a lady ought to go to Australia, and, as far as he knew, he thought she would be better in the part of Australia she mentioned than here. No doubt Dr. Moore's paper, with its valuable statistics, bearing on the subject of climatic influences, would help to guide the physician in advising his patients where to choose their residence. He agreed with the President that a comparison between the statistics of this country and those of Tasmania was not worth much. Here we had towns with dense populations, whereas in Tasmania the population of the towns might be considered as rural, the towns being open, without the overcrowding which existed among our urban population. He also thought that the comparison between the death-rate and the birth-rate in Dublin and in the Tasmanian towns was of very little value. While he believed the deaths were nearly all registered here, he thought a large number of the births were unregistered; and the disturbing cause in Dublin, emigration, was greater than the disturbing cause in Tasmania referred to by Dr. Moore, for more than 3000 people left the city of Dublin annually—a circumstance, which, of course, influenced the birth-rate and the proportion between that and the population. A comparison between the towns of Tasmania and some of the



American cities would be of more value. Tasmania was a very sparsely populated country, with a population not equal to that of Dublin scattered over a country almost equal in extent to Ireland. He should like Dr. Moore to say what the population per acre in Hobart Town was, as the per-acreage population is the most important disturbing cause of the death-rate.

DR. STEWART said that some time ago he made a trip to America, and was amazed at the influence which the climate had upon him. In Canada and in the United States he felt great buoyancy, and he attributed that to the clearness of the atmosphere. In those countries, winter was the pleasantest time of the year, if people took care to be well clothed.

DR. HENRY KENNEDY wished to know something from Dr. Moore in reference to the vegetables and crops that grew in Tasmania, as he thought that would have a great bearing on the vital statistics of the country. With reference to the number of persons who attained a great age, he believed there were parts of England—Devonshire, if he recollected rightly—where there were numerous and remarkable instances of longevity, and that consideration might make people cautious of running away to so great a distance as Tasmania.

The PRESIDENT thought it likely that the information Dr. Kennedy asked for as to the natural-growing crops of the country might throw some light on the question. In considering vital statistics of this kind they must draw a great distinction between a newly-settled and an old country. The people who went to settle in a new country were generally persons of a certain energy of character, with power to labour, and to raise themselves, and were favourable specimens as to questions of vitality compared with older populations. Those who went away were the strong and healthy; those who remained, the weak and debilitated. Besides, in a country like Tasmania, the care of the mother must be taken into account in considering the birth and death-rates. Persons who were tolerably well to do will take care of their children very differently from what was to be expected from the dregs of town populations in this country. He merely threw out these observations to show the difficulty that existed in drawing an inference from the low birth-rate in one and the high birth-rate in another country—from the low mortality in one and the high mortality in another. As a resort for invalids, he thought they should have a high opinion of Tasmania.

DR. J. W. MOORE, in reply, said he should, perhaps, have mentioned that, although Tasmania was five-sixths the size of Ireland, yet only a

comparatively small portion of the country was inhabited. Vast districts near the western coast were covered with dense forests, and the mountains and table-lands in the interior were also without inhabitants. The population dwelt chiefly in the neighbourhood of Hobart Town, Launceston, and Campbelltown. A fair comparison might, therefore, be made between the death-rate of Tasmania and that of the healthiest districts in England. While the deaths in the latter were 15, in Tasmania they were but 13 or 14 per thousand annually. This remarkably low death-rate went far to show the healthy character of the climate, more particularly when the lessened fatality of bronchitis and phthisis—the two most destructive diseases in England—was taken into consideration. Dr. Grimshaw had very properly alluded to the influence of density of population on the death-rate. The population of Hobart Town was 20,000, scattered over a large surface; the houses being isolated and surrounded by extensive gardens or grounds. But in considering the death-rate of the city, it should be borne in mind that the sick from nearly all parts of the country, the destitute, and the intemperate, were gathered within its precincts. This factor had a very pronounced influence on the statistics of morbidity and of mortality of the Tasmanian capital. Dr. Kennedy had asked a question as to the vegetables and crops of the country. Tasmania was one of the most fertile lands on the face of the earth; so much so, that (strange as it might appear) it had, in consequence, failed in an agricultural point of view. The farmers, depending too much on the marvellous fertility of the soil, had sowed the land year after year with the same or various crops without manuring, or bestowing any special care upon it. The soil had thus at length become exhausted, and this had for the present almost destroyed the agricultural prospects of the country. In relation to a remark made by the President, it should be remembered that six-tenths of the existing population had been born in Tasmania.

*On the Use of Bromide of Potassium in the Treatment of Epilepsy.* By THOMAS HAYDEN, F.K.Q.C.P.I.; Physician to the Mater Misericordiæ Hospital.

Bromide of potassium, according to M. Guibert, was first used as a medicine in 1828, by M. Pourché, in the treatment of various scrofulous affections. In 1857 Sir Charles Locock announced that he had used it, with signal success, in the treatment of epilepsy; and in 1862 Dr. Robert M'Donnell recorded some striking examples to the same effect, the epilepsy having been associated with menstrual derangement, and expressed the opinion that it is in such cases the bromide will be found most efficacious in the treatment of epilepsy. Since that date it has been made use of in the treatment of this disease by numerous competent

observers, and with an increasing confidence, warranted by the multiplied testimony to its efficacy.

Dr. Clouston, in 1868, experimented on twenty-nine insane epileptics, giving them 50 grains of bromide of potassium thrice daily for thirty-eight weeks. The general result was that the fits were reduced to one-sixth of their average number. The dose of 50 grains, he thinks, should not be exceeded, and the full benefit of the salt may be obtained from 30 grains.

The three following cases will serve to illustrate my experience of bromide of potassium in the treatment of epilepsy :—

CASE I.—Kate H., aged twenty-five years, assistant-teacher in an elementary school, and very intelligent, had a mild epileptic fit (*petit mal*) at the age of fourteen, and about a year before menstruation. This was followed by several fits within the year, at unequal intervals, and of a more decided character. At the period of her first menstruation she had a bad attack; but for two years subsequently had no fit whatever. At the age of twenty-one she was greatly alarmed by the sudden and serious illness of her mother, and in the course of the following night had two severe fits.

From this time till I first saw her, in October, 1872, a period of about two years and a half, she had a severe epileptic fit at every monthly period, usually in the night, and after the cessation of the flux, which was regular as to duration and recurrence, but attended with considerable pain in the back. In the fits she entirely lost consciousness, bit her tongue and lips, foamed at the mouth, had stertorous breathing and was convulsed, and on the following day was drowsy and stupid. During the last six months of this period she had also a second fit in the course of each month; her memory was gradually failing, and on one occasion, whilst teaching the children, she forgot the Lord's Prayer. She had constant head-ache and diarrhoea, with tremor of the hands, and was afraid to walk in the streets or to sleep alone. Her hair is abundant, and of a dark brown colour, and presents some grey hairs dispersed through it. These, at first few in number, appeared after her first illness; but latterly, on the occasion of the more severe fits, they have been so multiplied that, on the following morning, the hair of the entire head had become nearly white. This change of colour gradually passed away, so that at about the mid-period between two monthly illnesses, the hair had resumed its natural hue. In October, 1872, 20 grains of bromide of potassium were given three times daily, and in this quantity it was continuously administered for about one month. The first favourable change noticed was cessation of the muscular tremors, which, by interfering with her writing and other functions as a teacher, had caused her great inconvenience. She likewise acquired more confidence, and no longer feared to be left alone.

After the first month the bromide was given in doses of 30 grains thrice daily, but only for a week preceding her monthly illness, which was the only time at which she now apprehended a fit. During the succeeding eleven months the medicine was given as last stated, with the addition of an occasional aloetic aperient. Within this period she had only six epileptic seizures—that is, a fraction more than one every two months. They were, however, of a very decided character. The interval preceding the last, which occurred in October, 1873, and was brought on by a quarrel with her admirer, was the longest she had experienced for three years and a half, and since that date she has had no fit. I saw this girl two days ago; she is now actively engaged in the performance of her duties as teacher in a public school; her monthly health is quite regular; her memory is good, and she informed me that she is to be married a fortnight hence.

I have already stated that when the fits occurred in quick succession, her hair, which is naturally of a very dark brown, with a few grey hairs interspersed, became, on several occasions, almost entirely white, resuming its natural appearance in the course of about a fortnight after the monthly period.

On the morning of the 21st of January, 1873, she had a very severe fit, and I saw her in the course of that day; her hair was remarkably changed in colour, presenting white and dark hairs in about equal numbers. The specimen which I now exhibit was obtained on that occasion. Her hair, as seen on Monday last, is deep auburn, with a few grey hairs, not more than one in five hundred, interspersed. During treatment at the full dose of 30 grains three times daily, she frequently felt as if “drunk;” her articulation was thick, her head reeled, and she staggered in walking. She was, however, quite recollected and self-possessed, and in no degree excited whilst in this condition, which passed away in the course of the ensuing night, and did not return provided the bromide was omitted. It was manifestly a state of bromism, induced by saturation with the salt, as the symptoms mentioned were exhibited only towards the end of the period of its administration in full doses.

I would not venture to pronounce this girl cured, or to declare that, to insure future immunity, a further use of bromide of potassium may not be necessary. Her condition is, however, greatly improved, both in regard to general health, mental and physical, and the postponement (may I hope indefinitely?) of the fits. She feels assured that in the bromide she has a certain means of controlling her disease, and the state of happiness induced by this confidence is manifested in her buoyancy of spirits and cheerful looks. The details of this case, so far as they are not the result of personal observation, were furnished by the patient, and verified in her presence by her mess-mate and constant companion, a most intelligent girl, also a teacher in the same school, and a sharer of her bed.



CASE II.—Maggie S., aged twenty-two, a native of the county Kerry, was admitted into the Mater Misericordiæ Hospital in March, 1873. Had been, for three years preceding that date, subject to “fits,” which usually occurred at the monthly period, but there had been intervals of three and of six months of freedom from them. She lost consciousness and bit her tongue in these fits. Menstruation was regular as to period, but insufficient in quantity, and attended with much pain.

She was treated with bromide of potassium, in 20 gr. doses, three times a day, and had 5 gr. of co. aloëtic pill every night. From March to June 30th, when she left hospital to return home, she had only one fit, and that occurred in the night. For several days preceding this occurrence, which took place at one of her monthly periods, she was moody, irritable, and taciturn, and on the day succeeding it was dull and stupid, and had a singular expression of hebitude.

Whilst walking in the street, on the day she left hospital, she had a very bad fit, in which she fell down and cut her face. This she attributed to over-exertion and fatigue. She was re-admitted into hospital on that evening, and remained under treatment with the bromide till the 4th of July, when she returned home. I heard of her not long since; up to that date she had been quite well, and had not had a fit; she had, however, occasionally taken the bromide for a few days preceding her monthly illness, owing to the occurrence, now very rare, of the former “warning” of a fit, and with the usual satisfactory result of preventing it.<sup>a</sup> During the latter period of her treatment the bromide was given, as in the former case, only for a week preceding the monthly illness, when alone fits were to be apprehended. This change was deemed necessary, owing the exhibition of symptoms of bromism, consisting of vertigo, tottering gait, indistinct articulation, and *green vision*.

CASE III.—A clergyman, aged about sixty, and subject to epileptic fits of the graver form, came under my notice in 1871. The fits occurred at irregular intervals, but usually there were two in the month, attended with total loss of consciousness, and occasionally with personal injury in the fall.

There was well-marked *arcus senilis*, the superficial arteries were atheromatous, and the heart occasionally intermitted in action.

This gentleman took bromide of potassium almost continually for the two following years, in doses, at first, of 20 grs., and afterwards 40 and 60 grs. three times a day.

It was found necessary to occasionally suspend the medicine, owing to the occurrence of vertigo and intermission of the pulse. The only other

<sup>a</sup> Since the date at which this communication was made to the Medical Society (14th January), I have learned that she omitted taking the bromide of potassium as directed, and that, at the last two monthly periods, the fits occurred as previously to treatment.

medicine given was an aperient from time to time, and quinine with strychnia during the suspension of the bromide.

The general result was that the fits were greatly mitigated in severity; he had not more than two of the graver form within two years, the others being of the character of *petit mal*, and occurring during sleep exclusively. He was made aware of their occurrence on the following morning by finding his bed wet, and by a feeling of drowsiness and stupor. The average interval between the fits was likewise much prolonged; it was usually about two months, and on one occasion amounted to four months. This poor gentleman derived much benefit from an occasional voyage across the channel, especially when his stomach was deranged, as it occasionally was, by the large doses of bromide. I lost sight of him in the summer of last year, and am, therefore, unable to trace his history further.

In reference to the preceding cases, and the subject generally, I would beg to make the following remarks:—

None of these cases would warrant the assertion that a cure of epilepsy had been effected, although in all three the condition of the patient has been greatly ameliorated, and in two of them, after an interval of three and six months respectively, there has been no return of the fits, whereas, previously to treatment, they were in one case of monthly, and in the other of fortnightly recurrence. Some examples of alleged permanent cure have been recorded, but in none of them had sufficient time elapsed after the suspension of treatment, to warrant their being so regarded. So much granted, it is, nevertheless, quite indisputable that bromide of potassium is capable of controlling epilepsy in a marvellous manner, considering the hitherto intractable character of that disease. Through its agency the fits are mitigated in severity, the interval between them is protracted, and the nutrition of the nerve-centres is promoted, as judged by the improvement of memory, and of self-confidence, and the cessation of muscular tremor on the part of the patient.

Drs. Anstie and Jackson are of opinion that its efficacy is limited to a reduction in the number of the fits, and a mitigation of their severity; with the exception of a single case observed by Dr. Anstie, they have not witnessed an example of cure, in the sense of long absence of well-pronounced fits, without the continued use of the medicine at short intervals.

This is likewise my experience; but surely, even if no more can be claimed for the bromide than this, it will not be argued that in the treatment of so formidable a disease as epilepsy, the inconvenience arising from the occasional use of a medicinal agent by which it can be controlled, and, with more or less of certainty, averted, is a penalty in excess of the advantage gained.

There is no justification whatever for the scepticism of Professor Binz, who in a recent publication calls in question the alleged therapeutic value of bromide of potassium in the treatment of epilepsy, urging that in the recorded cases of cure by means of this salt, the improvement in the condition of the patient *followed*, rather than was produced by its use. No doubt strict criticism and severe judgment are not only warranted but demanded in the investigation of the properties of a new therapeutic agent; but it is no less certain that, in matters of this kind, conclusions drawn from experiments made in the laboratory, or in the domain of inferior animal life, must be tested and confirmed by application to the diseased human body, ere they can be accepted as guides in medical practice. This is the weak point in Professor Binz's otherwise able memoir. He dogmatically sets aside bromide of potassium as a doubtful, if not a useless agent in the treatment of epilepsy, without adducing any clinical experience to justify this course.

As regards the dose of the bromide, I have rarely exceeded 30 grs. given thrice daily, and I am quite of the opinion of Dr. Clouston, that the full benefit of this salt may be obtained without exceeding this dose. In one of my cases 3i. doses were given for several months consecutively, with occasional short intervals, but I was unable to perceive any advantage from the latter as compared with the smaller dose of 30 grs. Both were competent, but in different individuals, to produce the early symptoms of bromism, and this, in my judgment, should constitute the limit to be attained, and should not be exceeded.

Of the many symptoms of bromism which have been mentioned by writers, the foregoing cases presented only three—namely, vertigo, indistinct articulation, and an unsteady and tottering gait, as if the person were under the influence of alcohol. I have not witnessed hyperæmia of the palate, swelling of the mucous membrane of the mouth and nose, salivation and lachrymation, a feeling of heat and burning in the epigastrium, mental confusion and drowsiness, depression, failure of memory, an acneiform eruption, and complete anæsthesia of the palate and pharynx. These are, I doubt not, symptoms of a more advanced stage of bromide intoxication, which, for curative purposes, it is, I think, unnecessary to attain. The *green vision*, however, experienced in one of my cases, is worthy of special notice, as being, so far as I know, the only recorded example of this phenomenon, as a subjective symptom of bromism.

The PRESIDENT agreed with Dr. Hayden, that they ought not to reject a remedy because it did not produce a permanent cure. The relief to a sufferer from epilepsy, to the extent mentioned by Dr. Hayden, was like an exchange from earth to heaven. He himself had met with one very remarkable case in which the bromide had done wonders, though it might

not have caused a cure. An elderly lady, insane, used to have five or six fits every ten days before she came under his care. She had not had one for the last four or five months, and there was a considerable improvement in her mental condition. She was taking the medicine still, but in reduced doses. He did not think there was anything of an uterine nature connected with the fits in this instance.

DR. PURSER thought that one of the most remarkable points in the action of bromide of potassium was that it diminished or stopped the epileptic attacks, whether these depended on organic disease in the nerve centres, on peripheral irritation, or on mere functional derangement. In Dr. Clouston's cases the disease might fairly be supposed to depend on organic changes in the brain, and yet the fits were greatly relieved by the use of the bromide. Dr. Purser had prescribed bromide of potassium in a great many cases of epilepsy, and with an uniform result—namely, a diminution in the number of the fits, or their complete arrest during the use of the drug, and their recurrence on its suspension. He had at present under observation two cases in which this was noticed. In one, a girl, the fits occurred generally about the period of menstruation, but sometimes in the intervals; in her the use of bromide of potassium in 20 gr. doses three times daily, completely stopped the fits for upwards of a month. On stopping the medicine the fits recurred, and on resuming it they again ceased, and there has not been an attack for the last three months. In the other, a boy, there was some suspicion of intracranial disease, chiefly founded on the extreme hyperæmia of the optic discs. He remained free from epilepsy for a month, while taking bromide of potassium in 15 gr. doses. When he stopped the medicine the fits recurred, and again ceased when the medicine was resumed. The only ill effects Dr. Purser had seen from the use of bromide of potassium were eruptions of acné and slight drowsiness.

DR. QUINLAN mentioned a case confirmatory of what Dr. Purser had said of the efficacy of the bromide in relieving epilepsy arising from affections of the brain. During the last few months they had had a patient in St. Vincent's Hospital who was suffering from a depressed fracture of the skull. He had received an injury on the right side of the head, and a number of small pieces of bone were removed. He had at the time a number of epileptic fits, and latterly he had fits every week. At one corner of the depression of the skull there was a small piece of bone detached, and when he coughed the brain used to rise and fill up the depression. For a time there was also a distinct pulsation to be felt. On coming into hospital the patient had a fit. At first it was proposed to trephine him, but he hesitated to resort to that formidable operation. He then gave the patient bromide of potassium, and there had been no



recurrence of the seizures. He had no doubt that as long as the man took the bromide he would be free from the fits.

DR. GRIMSHAW had had a considerable number of epileptic cases under his care, and in a great number of them he had used the bromide of potassium. The history was the same as had been described by preceding speakers. They all improved more or less under the treatment. Most of them, as was usual with hospital patients, were lost sight of, but a few he had had under observation for some years. He had seen it demonstrated frequently that the drug prevented the fits as long as it was continued, but it had no permanent effect. One of these cases was that of a girl, an inmate of the hospital. She was treated with bromide, and as long as she took it regularly she had no fits, and appeared to be perfectly well. She was appointed as ward-maid in the hospital, and kept under observation for a considerable time. During that period she had only one fit, but that alarmed her, and the nurse of the ward objected to employ her any longer, and she passed from under observation. It was found that while she was acting as a ward-maid she had ceased to take the medicine for about three weeks, and to that he attributed the recurrence of the fit. Another case was that of a military man, who, he knew, had no epileptic attack for two years. He had rather a bad family history with regard to nervous affections. He got an attack of intermittent fever when stationed in South Africa, and was attacked with epilepsy. The affection was unilateral when he (Dr. Grimshaw) saw it, but both sides were affected when the disease first attacked him. He was seen by a physician in London, who gave him bromide, which apparently controlled the fits. The medicine, however, was given up soon. The patient was then seen by Sir William Gull, who again directed the use of bromide, and said he could go on for ever with it. He followed Sir William Gull's directions while in the neighbourhood of London. He then came to Dublin, and had an attack of epilepsy. Dr. Grimshaw being called in put him on bromide of potassium, giving him 40 grains twice a day. Within a few days after the patient came under his care he had an attack of epilepsy. From that time he had no attack whatever, until, without consulting anyone, he stopped taking the medicine, the result being that he had another epileptic attack. He had continued the use of the drug ever since that time, and had not had another attack. At the time he first came under observation he was falling into the condition of a confirmed epileptic. In all the instances in which he discontinued the use of the drug the fits returned. Dr. Grimshaw did not think he could say that a permanent cure had taken place in any case.

DR. HUGHES said the experience he had had coincided entirely, he might say, with that of the gentlemen who had preceded him, but he

thought they must look to larger and more extended experience for an accurate knowledge of the value of the drug. Individual experience was not sufficient to enable them to pronounce decidedly on the subject. He had had many opportunities of trying bromide of potassium, and in every case it had had the effect of lessening the frequency and the severity of the epileptic attack. In one very remarkable case, that of a young girl eighteen or nineteen years of age, who used to have fits seventeen times a day, they were reduced to one in a fortnight when bromide was taken. He had, however, never given it in the large doses he had heard spoken of that night. His practice was to keep the patient under the influence of the drug in small doses of from ten to fifteen grains. He could not say there had been a permanent cure in any case.

DR. HENRY KENNEDY asked what was Dr. Hayden's experience of bromide as a hypnotic? He had known elderly persons take ten grains as a hypnotic, and they found it quite sufficient to produce the desired effect. A late writer had stated that the potash was the active portion of the bromide, but Dr. Kennedy did not believe this, for potash *per se* had been given without producing any of the effects of the bromide.

DR. DARBY said he had had a large number of epileptic patients under his care. He always sent them to the Richmond Asylum, for he looked upon them, generally speaking, as incurable. Before bromides were introduced he had seen cases of epileptic fits following one another with great rapidity, then ceasing for months, and again recurring. Remembering these facts, the cases detailed that evening as having been benefited by the use of bromide of potassium lost their effect on his mind. He might mention that he had tried bromide of ammonium with young females who were affected with these fits during menstruation, and he thought the results were more favourable than those obtained with bromide of potassium. Some years ago he had a man under his care in hospital who was so bad that they were obliged to make his bed on the floor. He lay there for a week without being able to take any food whatever, and bed-sores formed. He got no treatment, but he recovered without it, and as soon as he regained his strength the fits went away. He saw that man the other day driving an untrained horse, and he had been in the service of a farmer for eight or nine years. He had come into hospital several times before the occasion described, always in epilepsy; but since his recovery from the last remarkable attack he had had no recurrence of the fits, and from that day to this he never had had any medical treatment. He believed there were some cases which were certainly improved by the exhibition of bromide of potassium, and especially that females attacked during the menstrual period were benefited by bromide of ammonium.

DR. FITZPATRICK said that the preceding speakers had merely alluded to epilepsy as a general term, and did not dwell on its varieties of form. He considered there were differences even in the minor form of the disease, and that cases which merely occurred in sleep, those depending on uterine irregularities in the female, and on seminal irritation in the male, were most amenable to medical treatment. In such cases he found marked benefit from the use of bromide of potassium, but time alone could determine whether a perfect cure was established. It must be borne in mind, that in cases of weak heart, or of embarrassed inspiration, the remedy was not free from danger. In the Lunatic Asylum to which he was physician, cases of epilepsy were not admitted, from present want of accommodation. He had, however, extended observation of the action of bromide of potassium in maniacal excitement; it certainly had some calming influence, but it produced no beneficial effect on the cerebral disease, and its action in procuring sleep was uncertain.

DR. HAWTREY BENSON said that as a remarkable clinical confirmation of the now accepted views as to the influence of bromide of potassium on the vaso-motor system, he would mention a case which occurred under his observation some years ago, at a time when the physiological effects of the drug were not so fully known as they are now. A girl aged eighteen years came to the City of Dublin Hospital, suffering, on an average, about two attacks of epilepsy every month. He put her on 15 gr. doses of bromide of potassium three times a day. At the end of a fortnight she returned, and reported that she had had *seven* attacks during that interval. He then put her on iron, quinine, and strychnia for a month, during which time she was seized but *once*. At the end of the month he again put her on the bromide, and in the following fortnight she was attacked *eight* times. He next went back to the treatment which had agreed so well with her before, and at the end of a month returned for the third time to the bromide, with a similar result, *six* attacks within the fortnight being the consequence. The secret of the injurious effect in the case was that the patient was highly chlorotic, and the bromide of potassium, acting on the vaso-motor system, increased that tendency to anæmia of the nerve-centres which already existed.

DR. HAYDEN, in reply, said the three cases he had described represented, substantially and fairly, the result of his experience of a considerable number of cases which he had treated with bromide of potassium. He could not agree with Dr. Darby's observations as to bromide of ammonium. He thought it was precisely and especially in cases of epilepsy, associated with uterine derangement, that the bromide of potassium was efficacious, and the bromide of ammonium not so. In cases of whooping-cough, he had given large doses of bromide of ammonium (ten

grains) with beneficial results. In reply to Dr. Kennedy's question, he had to say that he had found bromide of potassium act satisfactorily as a hypnotic, given in 15 or 20 grain doses, and in combination with an equal quantity of chloral. He was quite of the opinion that where there was organic disease of the heart, caution was required in giving bromide of potassium; but in one case, where there were decided indications of a diseased heart, he gave the patient 60 grain doses, three times a day, for over a year. Occasionally it was deemed necessary to suspend the medicine when the pulse became weak. Dr. Benson had mentioned a case of epilepsy in which bromide of potassium failed. That was a case of chloro-anæmia, and, no doubt, in such cases the drug would not prove efficacious till iron had been previously and sufficiently administered. He hoped the suggestion of Dr. Hughes, that bromide of potassium should be tried on a large scale in our lunatic asylums, would be acted upon. He appreciated Dr. Purser's observations as to the physiological action of the drug, and considered that he had done valuable service by his inquiries on that subject; but a reference to the results he had obtained did not probably come within the scope of this paper, which was purely clinical. There were, no doubt, certain cases in which bromide would fail, and he trusted that by and by they would be able to indicate those cases in anticipation. Up to the present he had not met with one such—every case in which he tried it having, in a greater or less degree, yielded satisfactory results.





# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

## THIRTY-SIXTH ANNUAL SESSION.

*Saturday, 10th January, 1874.*

LOMBE ATTHILL, M.D., V.P., in the Chair.

The Vice-President (DR. ATTHILL) exhibited specimens illustrative of the different forms of uterine polypi. The first specimen shown was that of a large mucous polypus. It was of the size of a man's fist. He pointed out that it illustrated three clinical facts. First, that these polypi may give rise to no symptom save profuse menstruation; secondly, the comparative rapidity of their growth; and lastly, their tendency to cure by a process of loss of vitality. Further, he would remark that both this and the second specimen illustrated the fact that intra-uterine polypi were generally met in women who had never been pregnant.

The patient from whom this polypus had been removed was an unmarried woman, aged twenty-six years. In January, 1871, she presented herself among the out-patients of the Adelaide Hospital, and stated that for the last few months menstruation had become very profuse. Suspecting the existence of a polypus, Dr. Atthill instituted a vaginal examination, but the uterus proved to be of the normal size and shape, and he contented himself with the administration of ergot and iron. This treatment proving of use, the young woman was lost sight of, but in April, 1873, she again presented herself. She then stated that the previous improvement had been but temporary, and that after an interval of some months, she relapsed into her former, or even a worse condition; that the flow had of late become still more profuse, and finally had been continuous for the last three weeks. She was then admitted into the Adelaide Hospital. On examining her then, a large soft intra-uterine polypus was found to exist. Its lower segment projected through the os uteri, which was as large as a five-shilling-piece. The sound penetrated to the depth of four inches into the cavity of the uterus. The patient was placed under the influence of chloroform; a wire was passed round the pedicle, and the tumour removed without difficulty; for though its size was so great, it being eleven inches in circumference, its texture was so soft that it was easily drawn through the os uteri. The lower portion of the tumour exhibited well marked signs of incipient decomposition, and Dr. Atthill

believed if the patient's strength had been able to bear the drain caused by the constant hæmorrhage, spontaneous cure would in course of time have taken place in this case by the loss of vitality and final expulsion of the tumour. Dr. Atthill was of opinion that the period embraced in the growth of the polypus could be, in this case, measured with tolerable accuracy. It had probably commenced to grow at the date of the first examination, January, 1871, but at that time its size must have been very small, for the uterus measured the normal depth of but two and a half inches. The tumour must then have taken about two years to attain its present size. Dr. Atthill pointed out that it was rare to find a case in which the growth of a polypus could thus be traced.

The second specimen was that of a fibrous polypus of the uterus. Dr. Atthill remarked that many practitioners failed to realize the difference which existed between a fibrous polypus of the uterus and a fibrous tumour of that organ when it became pedunculated.

The subject of the present case was also an unmarried woman, aged thirty-eight years. Her symptoms dated back so long as twelve years. She stated that for the whole of that period menstruation had been very profuse. She had been treated from time to time by the exhibition of various tonics and astringents, without any permanent good resulting. A vaginal examination had been made once, and the surgeon who examined her having pronounced the case to be one of cancer, she sought no further advice, till, in September, 1873, she consulted Dr. Atthill. At that time the menstrual flow usually lasted for fourteen days, the quantity of blood lost being very great, the interval between the periods being about seven days. On making an examination he found the uterus to be much enlarged, and to project forwards, the anterior wall being bulged out anteriorly. The sound proved the uterus to be four inches in depth; the os uteri was small, barely admitting the sound. Dr. Atthill having diagnosed the existence of an intra-uterine polypus, introduced seven pieces of sea-tangle, on the removal of which after the lapse of twenty-four hours, a firm tumour was found within the uterus. Round the pedicle of this a wire was successfully carried, and its attachment severed. Great difficulty was, however, experienced in extracting the polypus, for though the tumour was hardly half the size of the one previously exhibited, its tissue was so firm that it did not, as in the former case, mould itself so as to admit of its passage through the os uteri. It proved to be a true fibrous polypus of about the size of a large hen's egg. This patient also recovered rapidly. Dr. Atthill pointed out that both these tumours occurred in unmarried women, and that this fact had confirmed an opinion he had some time formed, that intra-uterine polypi were of far more frequent occurrence in nulliparous women than in those who had been pregnant.

The third specimen was one of true capsulated fibrous tumour, occurring

in the anterior lip of the os uteri. The patient was a servant, unmarried, aged thirty-one. She was admitted on the 17th of July, 1873, into the Adelaide Hospital. She stated that for some months previously she was conscious of a sense of weight and fulness in the vagina; that about six weeks prior to admission she discovered a tumour protruding from the vagina. This receded when she lay down, but always reappeared when she stood up or walked about. Menstruation continued to be perfectly normal.

On examination, an ovoid tumour of the size of a hen's egg was seen to project from the vagina, its long diameter being parallel with the vulva. In appearance this tumour strongly resembled the unimpregnated uterus. The sound passed upwards, and rather backwards, to the depth of  $2\frac{3}{4}$  inches, thus proving that the uterus, though drawn down, was still in the pelvis and but little increased in size. The tumour consisted of the anterior lip of the uterus, which was elongated and thickened; the uterus itself being drawn down by the weight of the tumour till it rested on the perineum, the os uteri being at the vulva.

The diagnosis of a fibrous tumour embedded in the anterior lip of the uterus having been made, Dr. Atthill determined to amputate the elongated portion of the cervix, electing to do so by means of the galvanic knife, hoping by that method to lessen the risk of hæmorrhage, which the thickened and hypertrophied condition of the part led him to think would be likely to occur. The apparatus employed was Grenet's. The galvanic knife consisted of a loop of platinum wire about half an inch in length, connected by means of the ordinary wire conductors with the battery.

The cervix measured at the point selected for amputation,  $3\frac{1}{2}$  inches in circumference. The great thickness of the tissue to be divided, and its extreme denseness, rendered the operation very tedious; it occupied in all thirty-five minutes. The slow progress made in dividing the tissues was also in no small degree due to the vascularity of the part, the flow of blood on each stroke of the knife being so considerable as to cool the platinum wire to such a degree, that ten or fifteen seconds frequently elapsed before the knife was hot enough to be again used. The cauterization was, however, sufficient to prevent any serious hæmorrhage occurring; still two arteries had to be ligatured. Excepting what occurred from these two vessels, the cauterization effected by the knife was sufficient to check the hæmorrhage, nor was there any subsequent loss of blood. The stump of the cervix cicatrized very slowly, and did not heal perfectly for more than two months. The operation itself was perfectly successful, but the great length of time which it occupied more than counterbalanced the advantage obtained in lessening the risk of hæmorrhage. The smell, too, caused by the slow cauterization of the parts was most disagreeable. As to the amount of pain caused by the galvanic knife as compared with that

produced by the ecraseur, the patient being under the influence of chloroform, no opinion could be formed.

On subsequent examination the amputated lip was found to contain a perfect fibrous tumour enclosed in its capsule.

Dr. Atthill said he had one other specimen to show, which he had removed from a patient on the previous Thursday. It was a specimen of cauliflower growth, of the cervix uteri. A remarkable phase of this woman's case was, that she had been under the care of two medical men, neither of whom had the least idea that she was suffering from uterine disease. She was admitted into the Adelaide Hospital for incontinence of urine, her only symptom being an incessant watery discharge, just sufficiently dark-coloured as to stain her linen a yellowish hue. This the woman herself, and the medical men who saw her, all supposed to be urine; but in reality it was the profuse watery secretion so often remarked in cases of cauliflower growth. Fortunately the growth was in an incipient stage, and Dr. Atthill decided to amputate the cervix uteri, and succeeded in encircling it with a wire, and in removing the entire cervix, and with it the whole diseased mass. The density of the tissue to be cut through was something extraordinary. He broke two strong pianoforte wires, and, finally, the ecraseur itself, in the attempt, and was obliged to obtain a new ecraseur and to use a still stronger pianoforte wire, in order to cut through the dense tissue. The woman was under the influence of chloroform during the operation. That was the third day since the operation, and she suffered from no symptom that would induce him to suppose any unpleasant consequences would follow. He wished to point out that though the cancerous growth had attained a considerable size, this patient was quite free from pain and from hæmorrhage, menstruation being perfectly normal.

*A Report of the Rotunda Lying-in Hospital for the year 1873.* By GEORGE JOHNSTON, M.D., F.K. and Q.C.P., Master of the Hospital.

GENTLEMEN,—The fifth year of my mastership of the Rotunda Lying-in Hospital having drawn to a close, I feel it incumbent upon me to continue, as I have begun, my annual reports of this valuable institution, and in so doing, permit me to state that I have adopted the same plan as on former occasions, of giving simply a plain statement of facts, just as they occurred, without note or comment, so that on your being made acquainted with them you may be able to draw a fair conclusion as to the real sanitary state of the institution.

You, no doubt, must recollect the attack that was made some years ago, and even is still carried on against large hospitals, and maternities in particular, as being the source and seat of epidemics, and consequently of great danger to the patients admitted to them. Statistics,



not in all cases accurate, nor from reliable sources, have been brought forward to prove the truth of these charges, and comparisons made of the mortality in small hospitals with that of such an institution as the Rotunda. It must be allowed that the value of statistics depends altogether on the accuracy of the data and on the unprejudiced manner in which they are framed. The tables I am about to give you have, as on all former occasions, been drawn out with the greatest possible care as to their truth in every detail. Each event was noted down at the bedside precisely as it happened, and in fatal cases the *post-mortem* appearances have been copied from the notes taken at the time the examination was made.

I have great reason to be thankful that, although our mortality may at first glance appear large, there has not been anything at all of the nature of an epidemic, notwithstanding that many such have prevailed in the city from time to time during the year. In some instances we had individuals coming in from houses where members of their families were struck down with scarlatina, fever, &c., and in all, the patients themselves made good recoveries, nor did they communicate any disease to others in the same ward. The number of zymotic cases has been (comparatively speaking) few, occurring in most instances at long intervals from one another, as may be seen by the tables, not confined to any particular ward; and in all such cases, there was sufficient reason to lead us to suppose that the cause of the illness, or death, when it occurred, was to be attributed to their state of mind, or previous delicacy of health, rather than to the circumstance of their labour having taken place in the hospital; that, in fact, the same result would have happened had they been confined at home (if they had any), or in any other place, even a cottage hospital. And here it may be well to remind you that patients of every class and in every condition are admitted to the Rotunda Hospital, if only they have the symptoms of labour. Some, for instance, have been brought to us in the last stage of exhaustion from hæmorrhage, or prolonged labour; some perfectly comatose in convulsions; others with ruptured uterus; some with all the symptoms of inflammation, of peritonitis, or phlebitis; some in a dying state from phthisis, or pneumonia, laryngitis, &c.; some with typhus, or typhoid fever; others again in a desponding state of mind from seduction, loss of husband, &c., as may be seen by referring to my past Reports, and as will be found in this. Surely, if a fatal result ensue in cases such as these, it is not to be attributed to the fact of their being confined in the Rotunda Hospital.

This immunity from epidemics, which has existed for the past five years, is mainly owing to the system of thorough ventilation carried out from time to time, the upper sash of a window in each ward being kept constantly open to the extent of six inches—to the

doing away with the old-fashioned canopies and curtains of beds—the insisting on the immediate removal of all excreta from the wards; the thorough cleanliness of the patients and their surroundings—the regular attendance on them night and morning, supporting their strength with nutritious animal diet, such as chicken broth, beef-tea, &c., judiciously given from the commencement—and not allowing labour to proceed until febrile symptoms appear, but by timely interference relieving the patient from an amount of suffering and exhaustion, and consequent danger, to which they otherwise would be liable to.

From all these circumstances we are able to keep our wards so pure and healthy that numerous visitors, many of whom were medical men, on visiting the institution, have expressed their surprise at the absence of an hospital atmosphere, and not a few have asked the question—“What disinfectant do you use?” We have been able to say that thorough cleanliness and soap and water were the only means adopted.

During the year ending the 5th November, 1873,

1,191 patients were confined in the hospital.

77        „        „        at their own homes.

286        „        were under treatment in the wards for uterine diseases.

2,814        „        were prescribed for at the dispensary, making in all

---

4,368 cases relieved.

Of the 1,191 patients who were confined in the hospital, 934 were purely natural labour, *i.e.*, the child presented by the head, and was born by the natural efforts within 24 hours.

In 39 the labour lasted over 24 hours; 15 gave birth to twins; 1 to triplets; 44 were abortions, *i.e.*, the ovum was expelled within 6 months; 2 of these were hydatids. In 5 instances the child presented with the upper extremity, and in 35 with the lower. In 142 cases we were obliged to effect delivery with the forceps; in 6 the perforator had to be employed, in all of which there was sufficient evidence that the child was dead previous to the operation. Version was performed in 16 cases. Labour had to be induced in 2 instances, owing to deformity of pelvis. 10 were admitted with accidental hæmorrhage; 3 with placenta prævia. There were ten cases of *post-partum* hæmorrhage, all of a trivial character, but 1 which required the injection of the solution of perchloride of iron. There were 12 cases where the placenta was retained. In 10 cases the labour was complicated with prolapse of the funis; in 4 with convulsions; 5 patients were epileptic; apoplexy occurred in 2 instances; mania in 8.

If the following list, showing the state and condition of the patient on admission is examined, we find 1 with phlebitis from inflamed varicose veins; 6 with symptoms of typhoid fever—enteritis; 2 with gastritis;

25 with bronchitis; 12 with laryngitis; 1 pleuritis; 3 pneumonia; 4 phthisis; 1 disease of the mitral valve; 1 renal disease; 3 ulceration of the vagina; 1 labia and perinæum in a state of ecchymosis; 1 purpura; 1 rubeola; and 8 cases of syphilis.

In addition there were 35 cases admitted, labouring more or less under great distress of mind, viz:—

6	were cases of seduction,	.	.	.	4	of which died.
17	„	where the husband had died	.	1	„	died.
8	„	„	had deserted them	1	„	died.
3	„	„	had beaten them	1	„	died.
1	a case of great distress from reverse of circumstances, husband having lost his situation,			1	„	died.
—				—		
35				8		

Surely when such cases as these are taken into account, the hospital should not be charged with being the cause of their death, should it occur, as the same result would, in all probability, have taken place had they been confined elsewhere. And here I can confidently say that, after five years' experience and close watching of 5,791 cases, in no one instance could a death be attributed to their labour having taken place in the hospital.

There were 39 cases where the labour extended beyond 24 hours, all of which had to be completed instrumentally.

6 of these were of 25 hours duration; 5 primiparæ.					
7	„	26	„	6	„
2	„	27	„	2	„
4	„	28	„	4	„
3	„	29	„	3	„
9	„	30	„	8	„
2	„	36	„	2	„
4	„	40	„	4	„
1	„	42	„	1	„
1	„	48	„	1	„
—				—	
39				36	

As all the above cases were delivered instrumentally—viz., 37 with the forceps, and 2 with the perforator and crotchet, they will be mentioned under the head of difficult labour in their respective classes.

#### TWINS.

We had 15 cases of twins, 4 of which were primiparæ; 11 pluriparæ.

In 6 instances both children were male, in 3 both female; all born alive but 1 female.

In 6 a boy and girl were the offspring; in 4 the boy came first, in 2 the girl. The first, a girl, was living; the second, a boy, was putrid.

In 1 the girl was dead.

All the mothers recovered, with the exception of one, who was seduced, and died from excessive grief.

#### TRIPLETS.

One patient, in her 11th confinement, gave birth to triplets. The first born was a girl, weight 6 lb. 4 oz.; the second, a boy, weight 5 lb. 8 oz.; and the third, a girl, weight 4 lb. 8 oz. Two of the placentæ were united, and one distinct. Mother made a good convalescence, and with her children went out well on the seventeenth day.

#### ABORTIONS.

We had 44 cases where the ovum was expelled prior to the seventh month, viz.:—

7 in the second month.

16 in the third month; 1 was a case of hydatids.

7 in the fourth month; 1 „ do. 1 twins.

2 in the fifth month.

12 in the sixth month.

—  
44

43 recovered; 1 died who had mitral valve disease.

#### PRÆTERNATURAL PRESENTATIONS.

In 5 instances, all pluriparæ, the child presented with the upper extremity all being the hand. 2 were males, 1 alive, the other dead, a case in which there was prolapse of the funis, in which pulsation had almost ceased when first examined. 3 were females, 2 living, 1 the second of twins, 1 putrid, owing to the mother being in extremely delicate health; she died of pneumonia; all the others recovered.

Version was performed in every case; in 4 under the influence of chloroform, and in 1, being the second of twins, it was not deemed necessary to use it.

#### PRESENTATION OF THE LOWER EXTREMITIES.

There were 35 instances where the child presented either with the breech or foot.

In the 8 primiparæ, 5 presented with the breech; 3 with the foot.

„ 27 pluriparæ, 23 „ „ breech; 4 with the foot.

Of the 5 primiparæ, presenting with the breech, 2 were boys, alive; 3 were girls, 2 of whom were alive; 1 was the first of twins, and 1 was putrid at the seventh month.



In the 3 primiparæ, where the foot presented, 1 was a boy, living; 2 were girls, also living; all the mothers recovered.

Of the 23 pluriparæ, presenting with the breech, 11 were boys; 1 was the first, and 2 were second of twins; 1 delivered under chloroform. All were born alive but 1, in which we were obliged to lessen the head before it could be extracted, owing to its large size (weight 10 lbs. 6 oz.), and the pelvis being small. 12 were girls; 1 was the first of twins; 1 was putrid.

Of the 4 pluriparæ, where the child presented with the foot, all were girls; 1 was born alive, 1 dead, owing to prolapse of the funis which was pulseless on admission; 2 were putrid.

2 mothers died, 1 a case of seduction; 1, her third pregnancy, of low fever—typhoid (enteritis).

#### FORCEPS.

In 142 instances we deemed it advisable to employ the forceps, in order not only to save the child, but to prevent serious consequences to the mother. 104 were primiparæ, 65 of which were delivered of male children, 39 female. 3 of the male children were *dead* at birth; 2 were cases of convulsions; 1 child was hydrocephalic; 2 were putrid; all the female children were alive.

Of the 60 male children born alive, 56 lived, 4 died; all the female children lived. Of the 38 pluriparæ, 22 were delivered of male children, 16 female; 3 of the male children were *dead* at birth, all of which were cases of prolapse of funis; all the female children were born alive. Of the 19 male children born alive, 2 died; 1 where the mother was brought in in convulsions; and 1 a case of disproportion, with early rupture of the membranes; length of labour, 24 hours. 16 female children born alive, 1 died, labour being induced at 7 months.

*Cause of Interference.*—70 were in consequence of inertia; 39 disproportion; 3 projection of promontory of sacrum; 1 projection of apex and coccyx; 2 approximation of the tuberosities of the ischia; 1 owing to an exostosis on the sacro-iliac synchondrosis; 14 from malposition of the head, viz., 4 in the 2nd position, 7 in the 3rd, 3 in the 4th, and 2 brow-presentation. In 1 the labour was induced at the 7th month, owing to diminutive pelvis; in 4 the funis was prolapsed; 4 in consequence of convulsions, and in 1 from epilepsy.

Of the 142 cases, 132 mothers recovered, 95 being primiparæ, and 37 pluriparæ; 10 died, 9 being primiparæ, and 1 pluripara. 2 died of peritonitis, 1 primipara, being a case of seduction, and 1 in very delicate health and anæmic on admission. 2 of pyæmia, both primiparæ, 1 a case of seduction; in the other there was great delay in the first stage, with early rupture of the membranes; head arrested high in the cavity, owing to large size of child, which weighed 8 lbs. 2 ozs.; besides, being in the 4th position, there was great difficulty experienced in extracting it. Unfavourable symptoms set in on the 4th day, and she died 4 days after.

4 died of convulsions, 3 being primiparæ, and 1 pluripara. 1 of the primiparæ, on *post-mortem* examination, showed evidence of acute Bright's disease, without any peritonitis or metritis. In the pluripara there was embolism of the right pulmonary artery, with disease of the kidneys and both ovaries.

1 died of gastritis, which she was suffering from on admission, a primipara. 1 of laryngeal phthisis, admitted with great dyspnœa.

TABLE NO. 1.—Cases where the Forceps were used before the Os Uteri was fully Dilated.

No.	Age	No. of Pregnancy		CHILD						Hours in Labor	Cause of Interference					Degree of dilatation of os at time of operation			Amount of advance of the head when the forceps were applied			Result to Mother		Cause of Death	
				Sex		Weight	Lived	Died	Early rupture of membrane		Prolapse of funis	Con- vulsions	Epileptic	Two-fifth	Three-fifth	Four-fifth	Above brim	In the brim	In the cavity	Recovered	Died	Peritonitis	Con- vulsions		
		M.	F.																						
1	40	—	8	L.	—	lbs. oz. 7 2	L.	—	18	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
2	22	—	3	L.	—	6 8	L.	—	13	1	—	—	—	—	—	1	—	—	1	1	—	—	—	—	—
3	45	—	3	D.	—	6 6	—	D.	5	—	1	—	—	—	—	1	—	—	1	1	—	—	—	—	—
4	19	1	—	—	L.	7 14	L.	—	28	1	—	—	—	—	—	1	—	—	1	1	—	—	—	—	—
5	22	1	—	—	L.	6 0	L.	—	18	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
6	22	—	2	L.	—	8 4	L.	—	13	1	—	—	—	—	—	1	—	—	1	1	—	—	—	—	—
7	20	1	—	—	L.	5 2	L.	—	24	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
8	19	1	—	—	L.	9 0	L.	—	16	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
9	26	1	—	—	L.	7 4	L.	—	23	1	—	—	—	—	—	1	—	—	1	1	—	—	—	—	—
10	30	—	4	L.	—	7 4	L.	—	20	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
11	20	1	—	—	L.	7 15	L.	—	23	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
12	22	1	—	—	L.	7 0	L.	—	24	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
13	28	—	2	D.	—	8 8	—	D.	20	—	1	—	1	—	—	1	—	1	1	1	—	—	—	—	—
14	40	1	—	—	L.	7 10	L.	—	16	1	—	—	—	—	—	1	—	1	1	—	—	—	—	—	—
15	25	1	—	—	L.	9 12	L.	—	14	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
16	31	1	—	—	L.	7 8	L.	—	24	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
17	17	1	—	—	L.	6 0	L.	—	25	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
18	26	—	3	L.	—	7 14	L.	—	7	1	—	—	—	—	—	1	—	—	1	1	—	—	—	—	—
19	24	1	—	—	L.	6 12	L.	—	26	1	—	—	—	—	—	1	—	—	—	1	1	—	1	1	—
20	23	1	—	—	L.	8 2	L.	—	36	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
21	28	1	—	—	L.	7 0	L.	—	30	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
22	19	1	—	—	L.	7 0	L.	—	26	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
23	23	1	—	—	L.	6 3	L.	—	28	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
24	23	—	2	—	L.	7 4	L.	—	15	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
25	20	1	—	—	L.	7 10	L.	—	24	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
26	26	—	2	—	L.	8 14	L.	—	26	1	—	—	—	—	1	—	—	—	—	1	1	—	—	—	—
27	28	1	—	—	L.	5 8	L.	—	17	1	—	—	—	—	1	—	—	—	—	1	1	—	—	—	—
28	23	—	2	—	L.	6 8	L.	—	30	1	—	—	—	—	1	—	—	—	—	1	1	—	—	—	—
29	30	1	—	—	L.	7 6	L.	—	30	1	—	—	—	—	1	—	—	—	—	1	1	—	—	—	—
30	20	1	—	—	L.	8 10	L.	—	17	1	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—
31	22	1	—	—	L.	6 6	L.	—	29	1	—	—	—	—	—	—	1	—	—	1	1	—	—	—	—
32	22	1	—	—	L.	7 8	L.	—	30	1	—	—	—	—	1	—	—	—	—	1	1	—	—	—	—
33	36	—	11	L.	—	7 14	L.	—	16	1	—	—	—	—	—	—	1	—	—	1	1	—	—	—	—
34	—	1	—	—	D.	3 10	—	D.	—	—	—	1	—	—	1	—	—	—	—	—	1	—	—	1	—
35	23	1	—	—	L.	7 12	L.	—	17½	1	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—
36	20	—	2	L.	—	4 9	L.	—	12	—	—	—	1	—	—	—	1	—	—	1	1	—	—	—	—
		24	12	21	15			33			32	2	1	1	11	22	3	2	14	20	34	2	1	1	

There were 36 of the foregoing cases in which we considered it prudent to apply the forceps before the os uteri was fully dilated; and as there may be still many who will be astonished at this apparently bold mode of practice, and mayhap question its justifiability, I beg leave to assure them that, having adopted it for the last 2 years, during which time we delivered 71 such cases, we are more and more convinced, every day, of its great advantage in saving the lives of both mother and child. Of course, it is not without danger in unskilful hands, and should only be attempted by those who have thoroughly acquired that great delicacy of touch so essential in the obstetrician, and who have considerable experience in the use of the forceps; but if properly and carefully done, it is perfectly safe. It may be necessary to state, that in all such cases as we are speaking of, although the os is only dilated to the extent mentioned, it nevertheless must be *dilatable*. When the os is rigid, we adopt the usual means for its relaxation previous to operating. This practice not only saves the life of the child, but also prevents much danger to, if not the death of, the mother, from the effects of the long-continued pressure on the soft parts, particularly where the membranes have been ruptured, and the liquor amnii escaped early in labour. In order that you may see the results of the practice I give you a table, No. 1, showing the number of cases delivered, the special causes of interference, the amount of expansion of the os uteri at the time we began to operate, the absolute position of the child's head, whether above the brim, in the brim, or in the cavity of pelvis, and the result to mother and child.

*Cause of Interference.*—In 32 cases the membranes had ruptured, and the waters had escaped at the commencement of labour, and in most instances the cervix was thinly stretched over the head. 1 had patency of the aortic valve, and in 1 there was a projection of the sacral promontory, and a 3rd was quite anæmic, and extremely weak on admission, In 2 there was prolapse of the funis, which could not be reduced; 1 was a case of convulsions, and 1 epileptic.

*Amount of Expansion.*—In calculating the amount of expansion of the os uteri at the time of operation, 4 inches is assumed as being the utmost dilatation of the os uteri. This is divided into 5 parts,  $\frac{1}{5}$ th being about  $\frac{1}{16}$  of an inch,  $\frac{2}{5}$ ths 1 and  $\frac{5}{8}$  inches,  $\frac{3}{5}$ ths 2 and  $\frac{3}{8}$  inches, and  $\frac{4}{5}$ ths 3  $\frac{9}{16}$  inches.

In 11 instances the forceps were applied when the os was but  $\frac{2}{5}$ ths dilated, when, in fact, we were obliged to expand it with our fingers before we could pass the blades, and in every instance both mother and child were saved, with one exception, a case of convulsions, which was brought in comatose.

In 22 instances, where the os was  $\frac{3}{5}$ ths dilated, all the mothers recovered but one, and all the children but 2, which were cases of prolapse of funis.

In 3 instances, where the os was  $\frac{4}{5}$ ths dilated, the mothers recovered and children lived.

The *position* of the head with regard to the pelvis at the time the forceps were employed :—In 2 cases the head was above the brim ; in 14 in the brim ; and in 20 it was in the cavity.

*Result.*—All the mothers recovered but 2, 1 of which, a primipara, who was very delicate and anæmic on admission, died of peritonitis, with uterine diphtheritis ; the other, also a primipara, was admitted comatose, and in convulsions.

It was satisfactory to find that at the *post-mortem* examination of both these cases the os was not more fissured than would have occurred in natural labour, though in the first the os was but  $\frac{3}{2}$ ths, and in the second only  $\frac{2}{3}$ ths dilated at the time of operation.

#### CRANIOTOMY.

There were 6 cases in which we were obliged to lessen the head before delivery could be completed ; 4 primiparæ. In one the head was arrested above the brim, owing to its large size and the unyielding state of the bones. The forceps were applied, but all efforts to draw it down failed. The head was, consequently, perforated, when fully 8 ozs. of bloody serum escaped with the cerebral matter, and delivery was completed with the crotchet ; child, a male, 7 lbs. 8 oz. ; mother recovered.

*Post-mortem* examination of child :—The head was very large and very much ossified ; bones hard and thick ; fontanelles nearly obliterated ; sutures united ; dura and pia mater both much thickened and congested ; the longitudinal sinus was filled with firm, deep-red fibrinous structure for the length of half an inch, having all the appearances of inflammation of the cerebrum and meninges.

In 1 the brow presented. The forceps were applied, but not producing any effect, version was then, after a great deal of difficulty, performed and the lower extremities brought down, but it was impossible to bring the head through the brim, owing to projection of the promontory of the sacrum. The funis having ceased to pulsate, the head was lessened and delivery completed with the crotchet. The third was also rendered difficult, in consequence of the projection of the sacral promontory, besides being complicated with prolapse of the funis. The forceps were first applied, but failed in producing any effect ; pulsation having ceased, the head was perforated, after which the forceps had to be applied, and delivery was effected with considerable difficulty. The fourth was also owing to projection of sacral promontory. Version was performed with difficulty, in the first instance, but the head could not be drawn through the brim ; the funis having ceased to pulsate, perforation was performed behind the ear, and delivery completed with the crotchet. There was a deep depression on the right frontal bone of the fœtus. All the mothers recovered ; they were all delivered under chloroform.

Two pluriparæ ; 1 her fourth child. The difficulty was owing to an



exostosis of the horizontal ramus of the pubis of the left side, with cicatrix at the upper part of the vagina, the result of former difficult labours. The os remained perfectly rigid, notwithstanding the warm bath and opiates; it had to be incised before we could introduce Barnes' dilators, and as soon as possible we applied the forceps, but could not move the head, and as there was reason to believe the child was dead, it was lessened, and delivery effected after some difficulty with the cephalotribe. Mother died in thirty-eight hours from the shock.

The second, her ninth child, with small pelvis; all her previous labours had been difficult. In this instance the breech presented, which was arrested high in the cavity. There was also great œdema of the labia and perinæum. The breech was drawn down to the outlet with extreme difficulty, which was also experienced in extracting the arms. The forceps were then tried, but it was found impossible to apply them, both from the head being above the brim, and in consequence of the œdematous state of the soft parts. The funis having ceased to pulsate, the head was perforated behind the ear, and delivered with the crotchet. There was great narrowing of the antero-posterior diameter of the brim. The child, a male, weighed 10 lbs. 6 oz. Mother recovered.

#### VERSION.

There were 16 instances where version was performed. 3 were primiparæ, in 1 of which it was the second of twins; child lived; 1 a brow presentation, in which the forceps were tried ineffectually, owing to narrowing of the antero-posterior diameter of the brim, when, after turning, the head could not be brought down until it was lessened, and delivery completed with the crotchet. The third was owing to projection of the sacral promontory; waters had escaped fifty hours, when, after version had been performed, finding it impossible to extract the head, it was perforated behind the ear and delivered with the crotchet. All the mothers recovered.

13 were pluriparæ; in 3 of these it was second of twins. All were born alive, and all the mothers recovered.

3 were complicated with prolapse of the funis. The first, the hand presented, but the funis had almost ceased to pulsate when discovered; the child could not be resuscitated. The second was a case complicated with partial placenta prævia; the child lived for ten hours. The third was owing to deformity; the child lived; all the mothers recovered.

3 were in cases of placenta prævia, all of which were partial; all the children were born alive, but two died in a few hours after birth; all the mothers lived.

4 were cases of hand presentation. 1, as you have seen, was complicated with prolapse of funis. The second, the child was born alive, but died shortly after; mother recovered. Third, the child was putrid,

attributed to the great delicacy of the mother, who died twenty-two days after delivery from pneumonia, which she was suffering from on admission. The fourth, both mother and child lived.

There was one instance where the face presented, in which the forceps were tried ineffectually. Version was then performed, and delivery effected after a great deal of difficulty, owing to projection of the promontory of the sacrum. The child lived for about five minutes; the mother died on the fourth day. She had been suffering from acute bronchitis for a fortnight before admission.

On *post-mortem* examination there was sloughing ulceration through the cervix uteri, evidently the result of long pressure.

In one instance the labour was induced at the seventh month, her second pregnancy, her former labour being extremely difficult owing to a diminished pelvis; she recovered; child lived.

Thus it will be seen that of the 16 deliveries by version, 14 mothers recovered; and 2 died.

12 children were born alive, 6 male, 2 of whom died, and 6 female, 3 of whom died.

3 were dead at birth, 1 male and 2 female; and 1, a female, was putrid.

#### LABOUR INDUCED.

In two instances labour was induced, both at the seventh month; first owing to diminished pelvis, her second pregnancy, first labour having been extremely difficult, and had to be completed by the cephalotribe. On this occasion her child, a female, weighing 3 lbs. 4 ozs. was delivered by version and lived; mother recovered. The second, her fourth pregnancy; all her previous labours were extremely difficult; her last had to be completed with the cephalotribe. She was delivered, with the forceps, of a female child, which lived eleven hours; mother was going on well, and left on the fourth day at her own request.

#### ACCIDENTAL HÆMORRHAGE.

There were 10 cases admitted with accidental hæmorrhage, all being pluriparæ, viz. :—1 was her 4th pregnancy, 3 their 5th, 1 her 6th, 1 her 7th, 1 her 8th, 1 her 13th, and 1 her 15th pregnancy.

2 male children were born alive; 1 lived, the other died within 1 hour. 8 female children, 6 of whom were born alive; 5 of these lived, 1 died on the 3rd day.

2 children were dead at birth; in both instances the mother was admitted in a state of great exhaustion, having had hæmorrhage for 15 days, notwithstanding which they, as well as the other 8 mothers, recovered. 8 were delivered by the natural efforts; 2 were breech presentations.

## UNAVOIDABLE HÆMORRHAGE.

3 cases were admitted with hæmorrhage from partial placenta prævia, all pluriparæ; 1 her 5th pregnancy, 1 her 9th, and 1 her 10th. All the children were male, and born alive, but only 1 lived.

Delivery was effected in the 3 by version, under the influence of chloroform. All the mothers recovered.

1, her 10th pregnancy, was admitted in a state of very great exhaustion, as well from the hæmorrhage as from her being in an extremely delicate state of health previously. Her convalescence was slow in consequence, but eventually she made a good recovery.

## POST-PARTUM HÆMORRHAGE.

There were 10 cases of *post-partum* hæmorrhage. In 3 where the application of cold to the vulva and a dose of ergot were sufficient to restrain it. In 6 it was thought prudent to inject cold water into the uterus. In none of them, however, was the pulse materially affected, and all recovered but 1, a case of seduction, in which the hæmorrhage was very slight, and who was convalescing favourably till her 5th day, when her mother spoke somewhat harshly to her, after which she began to complain of pain, and on our evening visit we found her with all the symptoms of peritonitis, which resisted our treatment, and she died on the 6th day of the attack.

In only 1 case, her 2nd pregnancy, the hæmorrhage was to that extent as to require the injection of the solution of perchloride of iron. She made a good recovery, was up on her 6th day, and went out quite well on the 8th.

As perhaps it will interest you to give these cases more in detail, I may mention that 6 were primiparæ. Of these there were 3 where the 2nd stage lasted three-quarters of an hour, and the 3rd stage was completed in—1 in 25 minutes, 1 in 5 minutes, 1 in 7 minutes; 1 where the 2nd stage lasted 1 hour, and the 3rd stage was completed in 15 minutes; 1 where the 2nd stage lasted  $1\frac{1}{4}$  hour, and the 3rd stage was completed in 10 minutes; 1 where the 2nd stage lasted  $2\frac{1}{2}$  hours, and the 3rd stage was completed in 17 minutes. Of the 4 pluriparæ, there were 2 where the 2nd stage lasted  $\frac{1}{4}$  of an hour, and the 3rd stage was completed in—1 in 5 minutes, 1 in 10 minutes; 1 where the 2nd stage lasted 1 hour, and the 3rd was completed in 5 minutes; 1, a case of prolapse of funis; version was performed as soon as the os was sufficiently dilated; the 3rd stage was completed in 15 minutes.

And with reference to the question at present under discussion, as to the relative tendency to *post-partum* hæmorrhage in cases where the 2nd stage is of short duration, I beg leave to lay before you the following table, in order to show that, so far as our experience goes, and it is

considerable, a short 2nd stage has no influence in causing *post-partum* hæmorrhage:—

In 26 cases the second stage lasted 5 minutes—all were pluriparæ. No hæmorrhage followed.

In 30 cases the second stage lasted 10 minutes—26 were pluriparæ, 4 were primiparæ. No hæmorrhage followed.

In 117 cases the second stage lasted 15 minutes—101 were pluriparæ; in 2 there was slight P. P. H.; 16 were primiparæ. No hæmorrhage followed.

In 37 cases the second stage lasted 20 minutes—33 were pluriparæ, no hæmorrhage followed; 4 were primiparæ, no hæmorrhage followed; and in 1 of these cases there had been severe P. P. H. on a former occasion, but none in this.

In 209 cases the second stage lasted 30 minutes—177 were pluriparæ, no hæmorrhage followed; 32 were primiparæ, no hæmorrhage followed; 4 were twin cases—3 pluriparæ, 1 primipara, no hæmorrhage followed; 1 had P. P. H. on former occasions, none in this.

In 121 cases the second stage lasted 45 minutes—82 were pluriparæ, no hæmorrhage followed; 39 were primiparæ; in 1 there was P. P. H.; 1 case of twins, pluripara, no hæmorrhage.

In 146 cases the second stage lasted 60 minutes—98 were pluriparæ 1 P. P. H., required solution of perchloride of iron; 48 were primiparæ, no hæmorrhage followed; 1, a case of twins, pluripara, no hæmorrhage followed.

Thus, of 686 cases where the second stage was within an hour, there were but 4 of *post-partum* hæmorrhage, 3 being slight, and 1 requiring injection of the solution of perchloride of iron.

#### PLACENTA RETAINED.

In 12 instances the placenta was retained.. In 3 from morbid adhesions—1 her first pregnancy, 1 her third, and 1 her thirteenth; all recovered. In 5 from irregular contraction, viz.:—3 their first pregnancy, 1 her 5th, and 1 her 8th; all recovered. In 4 from inertia—3 their first pregnancy, and 1 her fourth; all recovered.

In the cases of morbid adhesion the placenta was removed in 1 after waiting 40 minutes, in the second 45 minutes, in the third 50 minutes.

In the cases of irregular contraction the duration of the third stage was 17 minutes, 30 minutes, 35, 40, and 45 minutes respectively.

In the cases of inertia the placenta was pressed off in 37, 40, 50, 95, and 120 minutes.

With these exceptions, we have found that under ordinary circumstances, where due and proper attention is paid to the contraction of the uterus, at the time of the birth of the child, and during the third stage, the placenta will be expelled in the great majority of cases within 15



minutes, as will be seen by the following table, viz. :—Out of 1,147 cases the placenta was expelled—

In 145 cases within 3 minutes.

„ 373	„ 5	„
„ 409	„ 10	„
„ 133	„ 15	„
„ 50	„ 20	„
„ 16	„ 25	„
„ 7	„ 30	„
„ 7	„ 40	„
„ 5	„ 50	„
„ 1	„ 95	„
„ 1	„ 120	„

I may here remark, that we impress upon the pupils, never to hurry a placenta, either by traction at the cord, or undue pressure on the fundus, but by maintaining a steady contraction to obtain a short third stage, by which, and by the proper application of the binder, we avoid *post-partum hæmorrhage*.

In fact, we may come to the conclusion that when the placenta is retained beyond 20 minutes, except in cases of morbid adhesion, that it is in a great measure owing to want of proper manipulation.

#### PROLAPSE OF FUNIS.

There were 10 cases where prolapse of the funis complicated the labour, 2 being primiparæ, and 8 pluriparæ. 4 were delivered with the forceps, 4 by version; 1, a first of twins, was a footling, and 1, which was a primipara, the head had to be lessened, owing to great projection of the sacral promontory. There were 8 boys, 4 of which were dead at birth; 1 putrid; 3 lived. 2 girls, both of which were dead at birth. All the mothers recovered.

#### CONVULSIONS.

4 cases were admitted in convulsions, 3 primiparæ, 1 her second pregnancy; all were in a comatose state, 1 having had 27 fits before admission, 1 7 fits, 1 pulseless, could not ascertain how many fits, or how long she was in labour, but, on making examination, the head was found on the perinæum; she was at once delivered. The fourth had 7 fits before admission. All were delivered with the forceps. All died. 1 had acute Bright's disease. The second had disease of both ovaries and kidneys. The third, the kidneys, on *post-mortem* examination, were found extensively diseased; and in the fourth, both kidneys and liver were diseased.

EPILEPSY.

November 8, 1872.—M. A. C., aged 38, ninth pregnancy, confined of a boy 7 lbs. 12 ozs., at 8.35 a.m., after a natural labour of 8 hours' duration, second stage lasting half an hour, third 5 minutes; no p. p. h. At 10.30 seized with a fit of epilepsy, to which she has been subject for years; conscious shortly after: second fit at 12 noon; third at 4 p.m.; fourth at 6.30 p.m.; fifth at 7.45, after which they ceased. Made a good convalescence. Discharged 8th day.

December 21.—E. M., aged 28, third pregnancy, confined 9.30 p.m. of a boy 5 lbs. 12 ozs., labour natural, 7 hours; three-quarters of an hour in second stage; placenta expelled in 3 minutes; no hæmorrhage. 2 hours after confinement had a fit; a second in 6 hours after first.

22nd.—3.30 p.m., had a third fit; fourth at 11 p.m., after which there was no return. Requested to be discharged on the 28th. The child had a fit on the 25th.

July 20.—S. D., aged 23, second pregnancy, confined 2.40 a.m. of a boy, living, after natural labour 6 hours, half an hour in second stage, third 5 minutes.

Day after.—When spoken to she could not reply directly, or recollect her name or place of abode. She appeared to be in a state as if recovering from a drunken bout.

26th.—Much improved; says she has been subject to occasional loss of consciousness for some time back; attributes it to a passion she got into some months ago; she feels a premonitory creeping in her skin previous to the coming on of the attack, which generally lasted about 10 minutes. She improved greatly, and requested to be discharged on the sixth day.

July 6th.—M. W., aged 17, first pregnancy, seduced. This case terminating in mania, will be seen under that head.

October 18.—T. B., aged 20, second pregnancy, attacked with a fit of epilepsy at 10 a.m. on the 17th, from which she soon recovered consciousness. Had a second fit at 7 p.m. Barnes' dilator was introduced at 9 p.m., and in 4 hours we were able to deliver with the forceps.

25th.—Symptoms of mania, which we found was in consequence of her being under the impression that we were about sending her to the Lunatic Asylum.

26th.—Requested to be discharged.

APOPLEXY.

November 21st, 1872.—M. C., aged 24, second pregnancy, confined of a girl, putrid, at 6 months (attributed to delicate health); child was born in the street close to the hospital; had been in labour for 24 hours. Placenta expelled in 30 minutes. No p. p. h.

23rd.—At 7.30 p.m. had a slight convulsive fit.

24th.—Was in a stupid state; pupils dilated, with strabismus of both eyes; hemiplegia of right side.

25th.—Strabismus improved.

27th.—Strabismus gone.

December 1.—Went out well.

May 6th, 1873.—M. H., aged 27, first pregnancy, confined of a boy 8 lbs. 4 oz., after a natural labour lasting 8 hours, second stage  $1\frac{1}{2}$  hour, third 5 minutes. No hæmorrhage.

10th.—Mania, accompanied with pain in the head; milk fully secreted.

11th.—Decubitus dorsal. Semi-comatose; left eye suffused; tongue clean and moist; pulse 130, small, compressible; abdomen soft; uterus well contracted. Sank rapidly, and died at 4 p.m.

#### MANIA.

B. F., aged 35; confined 16th January, 1873; labour natural; in great anxiety of mind, and sleepless for several nights; attacked with pyæmia on her tenth day, and in 2 days after with mania. Particulars of her case given in the Zymotic Deaths.

L. T., aged 23, first pregnancy, natural labour; confined 20th April; had a slight attack of mania on her fifth day, but she recovered shortly after, and went out on the eighth day quite well.

M. H., aged 27, primipara; confined 6th May; labour natural; some doubt as to whether she was married. Attacked with great pain in her head, and mania on the fourth day. She continued in a semi-comatose state, and died on the 14th May of serous apoplexy, as before mentioned.

M. W., aged 17; had been seduced by her cousin, who went to America; her labour was natural; in 3 hours after she had an epileptic fit; her consciousness returned, and she complained of great head-ache. Next day the child had a fit. On the fourth day she became maniacal, and for several days continued so. We were obliged to send her to the lunatic asylum on the sixteenth day. She eventually recovered.

M. D., aged 26, first pregnancy; confined naturally on 7th August. On her fifth day she became very violent. Pulse, 140. She was maniacal for 2 days, with intervals of sanity, and went out, at her own request, on her eighth day.

B. M., aged 23, primipara; delivered with forceps on 16th September, owing to malposition and arrest of head. Being unmarried, she fretted very much, and was attacked with mania on 21st. She had been very nervous for 2 days previously. The mania soon disappeared, and she went out on 4th October.

M. M'C., aged 26; delivered, after a natural labour, of her first child on 22nd September. On the 24th she complained of great head-ache,

which continued for a couple of days, and on the 28th she became maniacal. Next day she was better, and she went out on 2nd October.

#### RUBEOLA.

M. A. F., aged 43; thirteenth pregnancy; admitted in the second stage; child, a boy 7 lbs. 11 ozs., born shortly after; labour lasted 8 hours, third stage 19 minutes; to all appearance in good health; went on favourably till the morning of the seventh day, when we found her face flushed; pulse, 120; tongue white, furred, red edges; eyes suffused. On examination discovered her legs, and arms, and chest covered with rubeolar rash. She insisted on going out strongly against our wishes. Ascertained she went through the sickness favourably.

#### CHLOROFORM.

Was administered in 132 cases of labour, in all to full anæsthesia, and in all with satisfactory results.

#### DEATHS.

The total number of deaths, from all causes, amounted to 32, which would be an average of about 1 in  $37\frac{1}{4}$ . But, if from these we abstract the 17 accidental cases, if I may so call them, which, of course, could not be considered attributable to the air or atmosphere of the hospital, it will leave 15 which died of zymotic diseases. This would be an average of 1 in 70 of the total deliveries, and might, by the casual reader, be charged to the hospital; but when the details of these cases are carefully examined with an unbiassed mind, I think it will be seen that there was sufficient reason to show that the same results would have occurred, even though they had been confined in any other place.

It has been well ascertained, and our daily experience convinces us, that when a patient is labouring under any anxiety or distress of mind at the time of her confinement, the convalescence is sure to be, to say the least, precarious, and in the generality of instances the mental depression being more than the system is able to bear up against, it too often gives way, and death is the result, as you may see by examining the history of the different cases.

Thus, the first, a primipara, aged twenty-three, was admitted in a state of great nervous anxiety, which, as she stated, was owing to "*her husband being at sea.*" We have strong suspicion that she had been seduced. She was confined after a natural labour of 12 hours duration, second stage occupying  $1\frac{1}{2}$  hour. On the 5th day complained of abdominal tenderness; pulse quick, tongue dry, with sickness of stomach. On the 6th, some wandering, incoherent. 7th, diarrhœa set in, which could not be checked. She gradually sank, and died on the 18th day after the attack.



TABLE NO. 2.—Deaths from all Causes.

No.	Date of Death	Ward	Bed	Age	No of Pregnancy		Cause	Observations
					1st.	Subt.		
1	Nov. 15	8	87	23	1	—	<b>Puerperal Fever</b>	'Husband at sea;' very anxious; 5th day maniacal pleuritis.
2	Dec. 9	6	70	25	1	—	<b>Peritonitis</b>	Seduced: symptoms set in in 24 hours.
3	Jan. 13	8	87	17	1	—	<b>Bright's Disease, Convulsions</b>	Had 27 fits before admission; labour natural, premature, 7 months; <i>p. m.</i> examination showed kidneys diseased.
4	Jan. 15	6	67	27	—	4	<b>Phthisis</b>	Had been ill for last 6 months.
5	Jan. 19	2	Sm.Wd.	23	—	2	<b>Convulsions</b>	Admitted comatose, having had 7 fits; never rallied; <i>p. m.</i> examination showed both ovaries and kidneys diseased.
6	Jan. 31	7	Sm.Wd.	34	—	9	<b>Pyæmia</b>	Mania; had been a widow; married only 2 months; child at term; great anxiety, fearing her family would hear of it.
7	Feb. 3	7	Sm.Wd. 64	26	1	—	<b>Convulsions</b>	Admitted in a state of collapse, having had several fits.
8	Feb. 13	3	Sm.Wd. 30	21	1	—	<b>Mental Distress</b>	Seduced; extreme depression of spirits; had male her mind up to die.
9	Feb. 21	3	26	30	—	2	<b>Pyæmia</b>	Great mental distress, her husband having died a week before.
10	Feb. 18	3	24	19	1	—	<b>Phthisis</b>	Admitted in a state of great anæmia; cough for 9 months.
11	Feb. 18	5	49	25	1	—	<b>Gastritis</b>	Bad husband; admitted with gastritis, from which she had been suffering for some time.
12	Feb. 18	7	Sm.Wd. 62	29	1	—	<b>Pyæmia</b>	A case of tedious and difficult labour, owing to early rupture of membranes; large child, and head in 4th position.
13	Feb. 23	7	58	30	—	3	<b>Enteritis</b>	Distress of mind; deserted by her husband since 29th March, 1871.
14	March 3	6	66	36	—	5	<b>Pneumonia</b>	Bad husband; caught cold a month before admission; lying ever since.
15	March 5	1	4	21	1	—	<b>2nd Hæmorrhage Syncope</b>	4th day got into a great passion; 2nd hæmorrhage supervened, restrained by cold, but in 6 hours got sudden collapse and died.
16	March 7	3	24	30	—	2	<b>Sloughing of Cervix Uteri</b>	Unmarried, a servant in a brothel; face presentation, version; <i>p. m.</i> examination showed great softening of anterior wall of the cervix.
17	March 16	2	14	34	—	5	<b>Phlebitis</b>	Admitted with veins of left leg varicose, and greatly inflamed, with intense pain in left iliac region.
18	March 21	7	57	28	—	3	<b>Peritonitis</b>	Admitted in low spirits, fretting from reverse of circumstances.
19	March 26	3	23	17	1	—	<b>Peritonitis</b>	Seduced; went on well till her 5th day, when her mother scolded her; peritoneal symptoms immediately after.
20	April 19	12	111	29	—	5	<b>Phthisis</b>	Admitted with hæmoptysis and bad cough; tubercle.
21	April 16	8	84	30	—	5	<b>Shock</b>	Difficult labour, owing to small pelvis; had to deliver with the cephalotribe; died in 38 hours.
22	April 25	2	17	32	1	—	<b>Pyæmia</b>	Seduced; deserted; great distress of mind, and very delicate on admission.
23	May 4	12	113	20	1	—	<b>Peritonitis</b>	Deserted by her husband; symptoms set in 3rd day.
24	May 11	8	86	27	1	—	<b>Serous Apoplexy</b>	Seduced; labour natural; mania on the 4th day; fell into semi-comatose state, and died in 31 hours.
25	May 20	12	115	24	1	—	<b>Uterine Diphtherite</b>	Admitted in a state of extreme delicacy; forceps; died in 74 hours.
26	May 28	12	110	19	1	—	<b>Peritonitis</b>	Very delicate all through her pregnancy, from hard work and bad living.
27	July 1	12	110	19	1	—	<b>Peritonitis</b>	Seduced; child and placenta had been expelled 3 hours before admission; went on well till 5th day, when her mother scolded her.
28	July 13	6	67	24	1	—	<b>Laryngitis</b>	Seduced; laryngitis and bronchitis for 5 years.
29	July 21	3	23	23	—	2	<b>Disease of Mitral Valve</b>	Admitted with great dyspnoea; <i>p. m.</i> examination showed aortic and mitral valve disease; pulmonary apoplexy.
30	Sept. 10	7	Sm.Wd. 64	32	1	—	<b>Convulsions</b>	Admitted comatose, having had 7 fits.
31	Oct. 3	8	84	33	—	2	<b>Peritonitis</b>	Very delicate for some time; child 7 months, putrid.
32	Oct. 7	8	87	24	—	3	<b>Peritonitis</b>	Husband out of employment, in consequence of heart affection; 2nd day complained of sleeplessness, on the 5th became maniacal, and died in 24 hours.

The second, also a primipara, aged twenty-five, *seduced*. Her labour was slow in the second stage, owing to disproportion and inertia, head being arrested in the cavity. She was delivered, with the forceps, of a boy living, weight 8 lbs. Twenty-four hours after complained of pain and tenderness in the abdomen; diarrhœa supervened, which was persistent, and she gradually sank, and died on the 9th, at 2 p.m., her fourth day.

B. F., aged 34, 9th pregnancy; had been a *widow*, in good circumstances, and mother of 8 children; made the acquaintance of a man by whom she became pregnant, and after she was seven months in the family-way, married him. She was admitted in a state of great anxiety, fearing lest her children and brothers would know the real circumstances of the case, besides which a law suit was pending, in which a good deal of property was involved. She was confined, after a natural labour of 6 hours duration, of a boy, living, 8 lbs. 14 oz. Pulse was rapid from the commencement. On the 24th, 9 days after delivery, pyæmic patches appeared on right wrist and arm, and on the following day on the left ankle. Became maniacal on the 27th, and sank on the 16th day after the birth of her child.

M. D., aged 30; second pregnancy; admitted in *very low spirits* and extremely weak, her *husband having died* a week before, leaving her in a state of great privation. Was delivered of a girl, living, after a natural labour of 5 hours duration. She was put on light nutritious diet, but her great debility continued. Pyæmic patches appeared on left foot and leg on the 5th day, the right knee on the 6th, and afterwards on the left arm, and she gradually sank on the 25th day after confinement.

A. B., aged 29, primipara; labour tedious and difficult owing to disproportion; the head was arrested in the cavity, and she had to be delivered with the forceps, of a boy, living, weight 8 lbs 2 ozs. The day after, she merely complained of weakness; had passed water; there was some abdominal tenderness on the second day. On the third a pyæmic patch appeared on the left leg, and in the evening one on the right side. On the 6th day the left buttock and labium of same side became affected, and she died on the eighth day after her confinement.

K. M., aged 30; third pregnancy; breech presentation; was delivered of a girl, living, 7 lbs. 4 ozs.: *very silent*; difficult to find out where she came from, or anything of her history, except that her "*husband was in America*." On second day got an attack of secondary hæmorrhage, which was restrained. On the third enteritis with low typhoid symptoms supervened, and she gradually sank and died on the seventh day.

A. M., aged 34; fifth pregnancy; admitted with the veins of her left leg varicose and intensely inflamed, with great tenderness in the left iliac region; ascertained that she had been working hard up to the morning of admission, although suffering for the last fortnight. Her labour was

natural, only of five hours duration. The inflammation never subsided, and she died on the eighth day.

E. M., aged 28; third pregnancy; admitted in *very low spirits*, her husband being out of employment, consequently she was almost in a state of starvation. Her labour was natural, of 7 hours duration; was attacked with diarrhœa; on the fourth day, sleeplessness; peritonitis developed itself on the eighth day, and on the ninth pleuritis with great dyspnœa and hurried breathing, and she died on the 11th day after her confinement.

M. S., aged 17; primipara; *seduced*. Confined after a natural labour of 7 hours duration. Was going on favourably till the fifth day, when her mother, who had come to visit her, said some harsh words to her. Peritoneal symptoms set in immediately after, and she died in 6 days.

A. S., aged 32; primipara; admitted in *very delicate health*, suffering from cough, and with *great anxiety of mind*; had come from America. Her labour was tedious owing to inertia. She had to be delivered with the forceps. Third day abdominal symptoms appeared. On the sixth there was sloughing of the vagina, and in the evening a pyæmic patch covered the right cheek. On the seventh, patches of pyæmia appeared on both right and left elbows, and both thighs. She died same day.

M. R., aged 20; primipara; delivered of a girl, living, after a natural labour of 9 hours duration. This woman was *fretting greatly*, on account of her husband having deserted her. Peritoneal symptoms set in on the third day, and she died five days after.

M. B., aged 24; primipara; "*delicate* all through her pregnancy from *hard work and bad living*." Her labour was tedious and had to be completed with the forceps. On the third morning we found her pulseless, and she died in 3 hours after.

*Post-mortem examination*.—Peritoneal sac contained some muddy, semi-transparent fluid; slight peritonitis; uterus well contracted; no metritis, but the cervical portion was covered with patches of diphtheritic membrane.

M. S., aged 19; primipara; admitted in *very delicate health*, which existed from the commencement of her pregnancy. Her labour was slow in the first stage, owing to early rupture of the membranes, and lasted 24 hours, but was completed by the natural efforts. On the third day peritonitic symptoms set in, and she died six days after.

K. M., aged 19; primipara; *seduced*; child born, and placenta expelled 3 hours before admission; no hæmorrhage followed; in a very depressed and anxious state of mind. Symptoms of peritonitis showed themselves on the fifth day, and she died two days after.

D. G., aged 26; third pregnancy; was confined of a girl after a natural labour of 12 hours duration; ascertained that her husband had been out of employment for the last 9 weeks, owing to disease

of the heart, and that they were in a *very destitute state*; the mammary secretion took place on the second day; in the evening complained of sleeplessness, for which she was ordered an anodyne. On the third day symptoms of peritonitis set in. On the fifth, after her husband had visited her, became maniacal; on the following morning found her in a state of great prostration, and she rapidly sank and died in 8 hours.

*Post-mortem examination.*—Rigor mortis well marked; abdomen very much distended with flatulence; the usual signs of peritonitis were very evident, but the quantity of lymph was small, except over the surface of the uterus and right ovary.

#### ACCIDENTAL DEATHS.

Of the 17 deaths which took place from accidental causes, 4 were cases of convulsions. The particulars of cases are mentioned under that head.

*1 Mental Distress.*—A case of seduction, with extreme depression of spirits; age 21; labour natural, 6 hours' duration: delivered of twins—a girl, living, presented by the breech; the second, natural, was expelled in the membranes, was a boy, and putrid. Low feverish symptoms set in shortly after; could not console her in any way; said from her coming into hospital that “she must die.” Milk was secreted on the second day. There never was any abdominal tenderness, or tympany, and she gradually sank, and died, as she said, “of a broken heart.”

*4 Cases of Phthisis.*—M. K., aged 19, primipara; admitted in very delicate health; quite anæmic; cough for 9 months, with hæmoptysis; was delivered of a girl, living, 7 lbs. 12 ozs., after a natural labour of 11 hours' duration, on the second day. There was an attack of secondary hæmorrhage; became gradually weaker, and died on the eleventh day.

A. M'G., aged 29, fifth pregnancy; admitted in very delicate health, from which she suffered during the whole of her pregnancy, with cough and blood-spitting; tubercle in right lung; was delivered, after a natural labour of 5 hours' duration, of a girl, living, 6 lbs. 14 ozs. System gradually gave way, and she died on the thirteenth day after her confinement.

J. M'D., aged 24, primipara; admitted with laryngitis, aphonia, and dyspnœa, phthisis; suffering for last 5 years; labour had to be assisted with the forceps; gave birth to a boy, living, 7 lbs. weight; had to be supported in the semi-erect position. She gradually sank, and died on the eleventh day.

C. M'G., aged 27, fourth pregnancy; had been suffering from phthisical cough for last 6 months; admitted to the chronic ward from Blackrock 3 weeks previous to her confinement, suffering from extreme dyspnœa, with hæmoptysis. In her seventh month she was delivered of a



boy, living, 3 lbs. 6 ozs., after a natural labour of 7 hours' duration ; but she sank rapidly, and died on the fifth day. *Post-mortem* examination showed extensive deposit of tubercle in both lungs.

*Gastritis*.—B. M'C., aged 25, primipara ; admitted with great pain and irritability of her stomach, and tenderness on pressure in the epigastric region ; tongue red and glazed ; told us she has been suffering from this complaint for some months. Labour was tedious and difficult, owing to disproportion and the extreme pain in the epigastrium ; obliged to deliver her with the forceps of a boy, living, 8 lbs. Gastric symptoms continued, and she died on the eighth day.

*Pneumonia*.—M. W., aged 36, fifth pregnancy ; admitted with inflammation of lower part of left lung, having caught cold from exposure a month before, owing to the cruelty of her husband ; delivered, after a natural labour of 15 hours' duration, of a boy, living, 7 lbs. 4 ozs. Symptoms continued unabated. Diarrhœa supervened on the fifth day, and she died on the seventh day after her confinement.

K. C., aged 33, second pregnancy ; admitted in very delicate health ; bad cough ; in her seventh month ; hand presented ; she was delivered by version of a girl, putrid, 2 lbs. 12 ozs. Pneumonia continued, and although she rallied for a time, she eventually succumbed.

*Syncope*.—R. K., aged 21, primipara ; was delivered of a boy, living, 7 lbs. weight, after a natural labour of 6 hours' duration ; went on favourably till the fourth day, when she got into a fit of passion. A dash of hæmorrhage took place, which was quickly restrained ; her pulse was not affected. However, in 7 hours after she became suddenly very weak, fell into a state of collapse, and died in 4 hours. *Post-mortem* examination.—Uterus was found fairly contracted ; no clots ; no inflammation in either uterus, ovaries, or peritonæum ; blood was in a fluid state.

*Sloughing Ulceration of Cervix Uteri, with Acute Bronchitis*.—E. C., aged 30, second pregnancy ; admitted with acute bronchitis and great dyspnœa, from which she had been suffering for a fortnight ; her labour was difficult, owing to face presentation ; child, a girl, delivered by version after considerable difficulty, owing to a projection of the promontory of the sacrum ; labour lasted 15 hours,  $2\frac{1}{2}$  of which were occupied in the second stage. Dyspnœa and bronchial symptoms continued unabated, and she died at 6.30 a.m. of the third day. *Post-mortem* examination showed acute bronchitis, with sloughing ulceration of the cervical portion of uterus.

*Shock*.—M. M., aged 30, fifth pregnancy ; labour difficult, owing to exostosis on left horizontal ramus of the pubis. This case has been





June	Total Deliveries	8	-	-	11	-	-	15	-	-	12	-	-	-	11	-	-	9	-	-	8	-	-	9	-	-	31
	" Deaths																										-
July	Primipara Deliveries	5	-	-	3	-	-	4	-	-	3	-	4	-	3	-	4	-	-	-	4	-	-	-	1	-	-
	" Deaths																										-
	Total Deliveries	12	-	-	11	-	-	10	-	-	8	-	13	-	10	-	13	-	-	-	13	-	-	-	1	-	89
	" Deaths																										3
Aug.	Primipara Deliveries	6	-	-	8	-	-	4	-	-	4	-	6	-	2	-	7	-	-	-	7	-	-	-	-	-	39
	" Deaths																										-
	Total Deliveries	14	-	-	14	-	-	14	-	-	14	-	16	-	12	-	15	-	-	-	15	-	-	-	-	-	118
	" Deaths																										-
Sep.	Primipara Deliveries	3	-	-	7	-	-	7	-	-	4	-	7	-	4	-	5	-	-	-	5	-	-	-	-	-	40
	" Deaths																										1
	Total Deliveries	8	-	-	11	-	-	14	-	-	13	-	14	-	9	-	9	-	-	-	9	-	-	-	-	-	-
	" Deaths																										1
Oct.	Primipara Deliveries	5	-	-	3	-	-	3	-	-	9	-	5	-	3	-	4	-	-	-	4	-	-	-	-	-	39
	" Deaths																										-
	Total Deliveries	13	-	-	13	-	-	14	-	-	13	-	12	-	9	-	14	-	-	-	14	-	-	-	-	-	97
	" Deaths																										2
Nov.	Primipara Deliveries	-	-	-	-	-	-	1	-	-	2	-	2	-	1	-	2	-	-	-	2	-	-	-	-	-	7
	" Deaths																										-
	Total Deliveries	-	-	-	-	-	-	1	-	-	2	-	4	-	4	-	4	-	-	-	4	-	-	-	-	-	17
	" Deaths																										-
Total Deliveries in each Ward		133	-	-	134	-	-	142	-	-	141	-	147	-	129	-	139	-	-	-	88	-	-	-	-	-	1191
Total Deaths in each Ward		-	1	-	3	-	-	-	6	-	-	-	-	6	-	1	-	6	-	-	6	-	-	-	-	-	32
Total Deaths from Zymotic causes in each Ward		-	-	-	-	-	-	2	-	-	2	-	1	-	-	-	4	-	-	-	2	-	-	-	-	-	15
Total No. of Primiparae Delivered in each Ward		55	-	-	49	-	-	51	-	-	64	-	52	-	36	-	42	-	-	-	27	-	-	-	-	-	422
Total Deaths in each Ward		-	1	-	1	-	-	-	3	-	-	-	-	3	-	1	-	3	-	-	3	-	-	-	-	-	18
No. of Deaths from Zymotic causes		-	-	-	-	-	-	1	-	-	1	-	1	-	-	-	1	-	-	-	1	-	-	-	-	-	9





described under craniotomy, which see. She died in 38 hours of the shock.

*Apoplexy.*—M. H., aged 27, primipara, *seduced*.. Particulars of this case already given under apoplexy.

*Mitral Valve Disease.*—M. M., aged 23, second pregnancy; admitted in a state of great exhaustion, with intense dyspnœa; quite anæmic, with disease of the mitral valve; pains frequent; gave birth to a girl, living, 2 lbs. weight (in her sixth month). She died in 15 hours.

Now, in order to show in perhaps a clearer view the number of deliveries as they took place in each ward, together with the relative number of deaths that occurred in them, as well as their nature, whether zymotic or accidental, I refer you to the following table, No. 3, by which, on reading from left to right, will be seen the number of deliveries that took place in each ward, as well as the number of deaths, and their nature, that occurred during the month. The figures in black type represent the total numbers, and those in red are the number of primiparæ.

Thus, in the month of November the number of the deliveries in No. 1 ward was 12, 6 of which were primiparæ; the number in 2 ward was 11, 2 of which were primiparæ; the number in 3 ward was 8, 1 of which was primipara; the number in 4 ward was 8, 1 of which was primipara. 1 death, a primipara, of a zymotic disease, the nature of which will be ascertained by referring to Table No. 2.

Then, in the month of December you will see there was only 1 death, which took place in No. 6 ward, a primipara, of a zymotic type.

Again, in the month of January 11 were delivered in No. 1 ward, 6 of which were primiparæ; no death. 14 in No. 2 ward; 1 death, a pluri-para, of an accidental cause, the nature of which, by reference to Table No. 2 will be seen was convulsions.

As it would be tedious to go further in detail through the whole table, and, as from the explanation so far given, it will be easily understood, I will merely confine myself to the 2 zymotic deaths which occurred in No. 3 ward, one in the month of February, and one in the month of March, in order to show that the deaths depended on the state and condition on admission, rather than to anything of an epidemic nature—viz., the first took place on the 21st of February of pyæmia; she lay in No. 26 bed. The second took place on the 26th March; peritonitis; and lay in No. 23 bed. No zymotic deaths occurring in the interval, although four batches of patients of four each were confined, and on each occasion the same beds were occupied.

Then in No. 7 ward, in the same month, one took place in the private ward, No. 62 bed, a case of pyæmia, who was admitted in a *state of very*

*great anxiety*; the particulars have been already mentioned. The other was a case of enteritis; lay in 57 bed, large ward; was admitted in very low spirits (*husband being out of employment*), and suffering from pleuritis, with dyspnœa, and low typhoid symptoms.

Then again, in the month of May, where you find 3 zymotic deaths in No. 12 ward, it will be well to lay before you the circumstances attending each case.

The first was admitted *fretting*, having been *deserted by her husband*, and was confined on the 26th of April, and lay in No. 113 bed, in the room facing the south. Peritoneal symptoms set in on the third day, and she died on the 4th of May; all the rest of the patients confined at the same time, 5 in number, went out well.

The second was admitted in a state of *extreme delicacy*, on the 15th May; had been ill for the last 9 months, suffering, to use her own expression from "*hard work and bad living*" was confined on the 17th, and lay in 115 bed, in the same ward, and died on the third day of uterine diphtherite.

The third was confined on the 18th May. She had been in a *very delicate state of health* all through her pregnancy; lay in 110 bed in a room facing north, and she died on the 20th.

On the 2nd of June a case was brought in *pulseless from accidental hæmorrhage*. She lay in the bed 110, and went out well. Another batch of patients was confined on the 10th, one of which lay in 110 bed. All went out well.

On the 22nd a batch of four patients was confined, and all recovered but one, *a case of seduction*; the child and placenta had been expelled for 3 hours before coming in. She lay in 110 bed, and was going on well till her fifth day, when her *mother* came to see her and *scolded her*. Peritoneal symptoms set in, and she died on the third day after the attack.

When these circumstances are considered, it may be fairly allowed that the deaths were attributable to their own state, rather than to the locality where it took place.

And next, I draw your attention to Table numbered 4, which is divided according to the Poor Law arrangement and Registrar-General's reports, into districts. Numbers 1, 2, 3, on the north side, and 1, 2, 3, and 4, on the south side. Each district is subdivided into three columns. The first gives the number of deaths from zymotic causes, which occurred in the district. The second gives the number of patients who came from that district, and were delivered in the hospital; and the third the number of deaths which occurred amongst them, for each month during the year. However, although it may be interesting for reading, and for reference to enter into the details of each month, it is only necessary at present to give the sum total for the year.





Thus in No. 1 district, north city, 186 deaths took place from zymotic causes. 301 patients coming from that district were delivered in the hospital, out of whom one died.

In No. 2 north city district, 123 zymotic deaths took place. 298 patients coming from that district were delivered in the hospital, of whom 3 died.

In No. 3, north city district, 175 zymotic deaths occurred. 142 patients coming from that district were delivered in the hospital, of whom 3 died.

In No. 1, south city district, 232 zymotic deaths occurred. 31 patients coming from that district were delivered in the hospital of whom 1 died.

In No. 2, south city district, 104 zymotic deaths occurred. 115 patients coming from that district were delivered in the hospital, of whom 2 died.

In No. 3, south city district, 246 zymotic deaths occurred. 119 patients coming from that district, were delivered in the hospital. All recovered.

In No. 4. south city district, 255 zymotic deaths occurred. 157 patients coming from that district were delivered in the hospital, 5 of whom died.

Thus showing that deaths from zymotic diseases, to a great extent have taken place, in the various parts of the city, during the year. And although the patients delivered came from the various infected localities, and, in some instances, even from among their families, where members were laid down with sickness, still they did not convey the malady to any of the other patients in the ward; and also that epidemics may prevail outside, and still the hospital be perfectly free from them.

In conclusion, I have only to recapitulate what I said at the commencement, that my sole object in bringing forward these reports is that of ascertaining the real sanitary state of the institution; and by laying before you the different details, enabling you to decide whether a large maternity is a safe asylum for those seeking its advantages.

The CHAIRMAN wished to know what Dr. Johnston meant by "uterine diphtherite."

DR. JOHNSTON said he meant a diphtheritic deposit such as was seen on the back of the pharynx. This deposit was found in the uterus in the cases he had mentioned, and he used the term applied to it by a first-rate pathologist.

The CHAIRMAN said he had asked the question in order to clear the ground for those who wished to take part in the discussion. Dr. Johnston's paper was of the greatest possible value. No doubt some would consider it a mere matter of dry details, but it was full of interest to those who took an interest in the subject themselves. The statistics in former days were so incorrectly and vaguely compiled that there was no foundation on which to build a practical deduction. If fifty

deaths occurred in hospital, they were all set down as puerperal fever, whereas Dr. Johnston had clearly shown that out of thirty-two deaths, at least one-half had no connexion with the puerperal state. He need not say that death from disease of the heart or phthisis could not be considered as puerperal; and yet in former days, deaths from all causes, as occurring in hospital, were often quoted as being due to puerperal disease. Dr. Johnston had shown that where proper care was taken the deaths were not more numerous in hospital than in private practice. There were just two points which he wished to comment on. In one of the cases referred to by Dr. Johnston the placenta was retained for ninety-five minutes, and yet no bad consequences occurred. Not long ago, in a discussion in that Society, he asserted that disastrous consequences were sometimes caused by the haste with which the third stage of labour was terminated by injudicious practitioners. He believed a vast number of practitioners throughout the United Kingdom removed the placenta immediately. Dr. Johnston had taught his pupils to give time till the uterus had detached the placenta; but many practitioners, the moment the child was born, commenced to knead the uterus, and to pull the funis, till they succeeded in expelling the placenta, and the result frequently was, *post-partum* hæmorrhage. Here was a case recorded by Dr. Johnston where the placenta had been retained for ninety-five minutes without any unpleasant consequences, and he trusted that important lesson would not be lost on obstetric practitioners. He was almost inclined to regret that Dr. Johnston had met with but one case in which he thought it necessary to resort to perchloride of iron. There was, he knew, an unjust prejudice against it, and, therefore, he should like to see its value more thoroughly established; while, on the other hand, he congratulated Dr. Johnston on showing that *post-partum* hæmorrhage could be prevented by the judicious management of the third stage of labour.

DR. THOMAS MORE MADDEN said that all who recollected the Rotunda Hospital some years back, must be struck with the vast improvement that had taken place in its management within the last eighteen years. He recollected the hospital when its condition was more favourable to the propagation of epidemic disease than it is at present. If there were a puerperal poison capable of saturating the building in which it was long continued, the germs of the disease must have lingered in the curtains and old wooden flooring, and their removal must have been beneficial to the health of the hospital. Greater attention was also paid to ventilation, and the dietary of the patients had been improved since he was connected with the hospital. Of course all these measures were favourable to the arrest of epidemic disease. With regard to the greater number of deaths this year as compared with the previous year, it should be borne in mind that outside the hospital epidemic disease had been rife. In his own practice—

a limited one—he had met with two deaths during the past year, and comparing that with the few deaths that had occurred in so large an institution as the Rotunda Hospital, he thought it spoke volumes for the admirable way in which that hospital had been managed. With regard to the use of the forceps in the first stage of labour, he could not say they had heard anything very new. The stages of labour were merely artificial divisions made for the convenience of obstetric practitioners. Labour was one continuous operation, and the stages into which it was divided were artificial and for the convenience of the practitioner, rather than founded on physiological principles. In some cases the os uteri would be fully dilated, and yet the patient would not be so far advanced in labour as in other cases where there was no dilatation of the os. A curved forceps, such as was recommended by Dr. Barnes, was a powerful instrument, and it was required only when the head was above the brim, and to use it properly required a great amount of manipulative dexterity. In ordinary cases a short forceps was sufficient, but where the head was above the brim a more powerful instrument than Dr. Barnes' forceps was necessary, and it was too powerful when they did not require a strong one; but Dr. Johnston's experience with Dr. Barnes' forceps was so favourable that it almost disarmed criticism on this point.

The CHAIRMAN remarked, that as far as he was aware the only practitioners in Dublin who used Barnes' double curved forceps were Dr. Johnston and himself. The practice of the Dublin School was in favour of straight forceps. He had used Dr. Barnes' forceps with great success, and he considered it to be as powerful an instrument as they could desire to possess.

SURGEON-MAJOR JOHNSTON said there must have been a large number of cases that showed some symptoms of pyæmia or peritonitis. In his own small hospital, on the cottage system, he found it perfectly impossible to keep it without some cases of zymotic disease appearing; and although he had had only one death in three years, yet every now and then cases appeared of a serious nature. He should like to hear something from Dr. Johnston on this point.

DR. JOHNSTON said his only reason for not giving the zymotic cases was, that the milder cases recovered, and the fatal cases are given in the Report. In no one instance had the disease been propagated by contagion. They were all of a sporadic nature. A case of rubeola appeared in the hospital the other day. The woman went through the disease favourably. His reason for bringing out these Reports was to vindicate the sanitary state of the institution over which he had the honour to preside.

The Society then adjourned.

## PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. LYONS, President.

DR. BENNETT, Secretary.

*Fatal Incised Wound of the Abdomen.*—DR. BENNETT exhibited the viscera of a man engaged as a porter on the quays of Dublin, who was stabbed by a Greek sailor some weeks previously. The man was admitted, immediately after the injury, to Sir Patrick Dun's Hospital, while Dr. Bennett was going round the wards in the morning. It appeared that he had got into some altercation with the sailors, and one of the latter ran after and stabbed him with a knife in the side. He ran for a short distance after this and fell. He was brought into hospital almost pulseless. He was very drunk. He was agitated, pale, and collapsed, but still presented the full evidences of intoxication. The injury, though it had produced collapse, had not destroyed the alcoholic intoxication. His only reply, when asked what had happened to him, was that some bloody Greek sailor had stabbed him. When the man was stripped, there was found, above the posterior third of the crest of the ilium on the left side, such a wound as the blade of a sailor's clasp knife would inflict, about an inch in length, with nothing but the merest trace of hæmorrhage from it, and his clothes were scarcely stained with blood. He complained of great pain in the belly, and referred it to the region of the umbilicus. Almost immediately after being placed in bed he vomited nearly a quart of porter and whiskey mixed. The two fluids were easily recognizable, the porter as the mass, and the odour of whiskey along with it. There was no trace of blood in the discharge from the stomach. He continued almost incessantly to vomit; nothing could check it, and it was the prominent symptom he complained of. He lived for five days, his vomiting resembling, in some respects, that which occurs in cases of strangulated hernia. After the first two or three great discharges it diminished in amount. The slightest movement caused him to make an effort at retching, and the fluid drained out of the side of his mouth. He presented one very remarkable symptom—he was incessantly hungry. Whether this was a mental delusion or not, his constant cry was for something to eat or drink. Neither medicine nor food would stay on his stomach. On the morning but one after his admission, he was nearly in the same state as when brought to the hospital; he was almost pulseless—so much so that as he lay on his right



side (his constant position), when he raised his left arm it was nearly pulseless, while in the right arm, which lay in the bed, the pulse could just be felt, but it was too feeble and fluctuating to be counted. He continued to refer all his pain to the epigastrium, even to the day of his death. There was no escape of gas through the wound, or any tympanitis. The tenderness did not extend over any great region of the abdomen, but was strictly confined to the umbilicus. His pulse became more rapid, while it recovered nothing in strength, on the third day. There was no motion of any kind from the bowels; neither fæces or flatus escaped, nor did any of the nutritive injections administered by the rectum escape—every one was retained, and they seemed to produce no amount of restoration. He gradually became more and more feeble; he lost consciousness of those around him, and died without any material alteration of his symptoms. The bladder acted voluntarily and without any distress, and there was no sign of blood or of albumen in the urine. On two occasions, however, he vomited enormous quantities of blood. On the third morning, when Dr. Bennett came to visit him, he saw the basin, at the patient's side, half full of dark fluid blood, which had evidently, from its discoloration, been a long time escaped from the vessels, but there was no coagulation. That discharge of blood was replaced by bilious vomiting without any trace of blood. On the next morning there was another vomiting of blood of the same character. Even with the knowledge that a knife had penetrated the abdomen, he was at a loss to know what the cause of this vomiting was. The *post-mortem* examination, however, offered some explanation of it. It was found that two wounds had been inflicted by the knife on the descending colon. This involved a wound in the vessels of the mesocolon, and the track of the wound was along the left colic vessels. The knife, after passing through the colon, had entered the stomach through its posterior wall, and had apparently impaled the anterior wall against the sheath of the rectus muscle. Seen from the peritoneal aspect, the posterior wound was the size of the blade of the knife, while the mucous membrane, inverted into the opening, to a certain extent diminished its size. A small hole, closed on the peritoneal aspect by lymph which glued it to the abdominal wall in front, marked the place of passage of the point of the knife into the abdominal wall in front. The hole in the anterior of the stomach differed from that on the other side, in the mucous membrane not being inverted. In the left rectus muscle there was found in the fibrous tissue of the sheath a wound much larger than the wound in the stomach. This could only be explained by supposing that the wall of the stomach was pushed against the rectus, thus causing the division of the rectus to be larger than the stomach wound; the mucous membrane was divided to a much greater extent than the serous, the actual point of the knife only escaping through the latter. On opening the peritoneum below the umbilicus

there was a great escape of intestinal gas. The whole of the peritoneum was glued together by recent lymph; there was universal peritonitis and a great extravasation of blood into the sack of omentum. The sack of the omentum was distended by a great quantity of both fluid and coagulated blood. The posterior wound of the stomach seemed the more important of the two. He thought the explanation of the vomiting of blood on the two occasions mentioned, was that the blood from the cavity of the omentum had flowed into the stomach, and was thence discharged. There were no large vessels lying directly in the track of the wound, so that whatever blood issued from the stomach could not have come from the wound in its walls; the great quantity vomited, and the fact of the vomiting occurring but twice, and when the man was sinking, suggested that it was blood which had regurgitated from the sack of the omentum through the posterior stomach wound. No evidence of the escape of the contents of the stomach into the cavity of the abdomen could be found, but those of the colon were found in abundance. There was a low diffuse inflammatory action along the sheath of the rectus muscle, probably caused by the escape of some of the gastric contents into the areolar tissue of the sheath.—*December 6th, 1873.*

*Comminuted Fracture of the Patella.*—DR. T. E. LITTLE exhibited a specimen of fracture of the patella, which he thought worthy of being laid before the Society, inasmuch as fractures of this bone were not commonly to be met with in *post-mortem* examinations. He could give no history of the case, because he came across the specimen in superintending some operations on a dead body about which he had no information. One or two points, however, he could mention as to the pathology of the specimen. It was a fracture that occurred from direct violence. He inferred this from the existence of a small scar on the skin in front of the patella. As to whether this penetrated to the knee-joint he had no evidence, but he was inclined to think it did not. It was a comminuted fracture. There were four fragments of the patella. A fracture passed vertically almost through the middle of the bone, and the inner of the fragments so formed was divided into three by two transverse fractures. On the cartilaginous surface of the fragments there were clear evidences of chronic rheumatic arthritis. The cartilage was gone, and on the corresponding part of the femoral condyle, there was distinct evidence of the existence of the disease. The fragments were not much displaced. Between the fragments broken by horizontal fractures the union was fibrous and strong. Between the two lateral fragments the union was that known as ligamentous. They were merely united by a thickening of the portion of the quadriceps extensor femoris, which passed over them. On the posterior surface of the fragments there was a considerable amount of fibrous tissue, which passed across

and formed a considerable bond of union between the fragments of the inner side. It existed as a distinct membrane which could be raised off the cartilage.

With reference to this fracture, there were two points of some pathological interest. The first was the existence of that change which was a consequence of chronic rheumatic arthritis. He examined carefully almost every other joint in the body, and in no other joint was there any evidence of that affection. This bore out the point remarked on by Mr. Adams in his work on this subject, that an injury of the joint acted in some way as a producing cause of that peculiar change of structure.

The second point of interest was the bands which were found lying on the free articular surface of the bone, and which he thought formed a bond of union between the fragments preventing displacement. He did not find these alluded to by any writer on fracture of the patella. There were rare instances of bony union, and in this instance they saw the fragments kept together by what he might call true fibrous union, and there was also in this case ligamentous union between the two lateral fragments formed by the extension of the quadriceps extensor. This latter form of union was admirably described by Mr. William Adams, in his paper read before the Pathological Society of London, but he made no allusion to the mode of union seen in this specimen—that is, a false membrane, organized, as he supposed he might say, and diffused on the free surface of the cartilage. This, he thought, was a point of interest, both in a pathological and a clinical sense.—*December 6th, 1873.*

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

### CONTENTS.

---

THIRD SERIES, No. XXVII.—MARCH 1, 1874.

---

#### PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. VI.—Select Clinical Reports. By ARTHUR WYNNE FOOT, M.D., Junior Physician to the Meath Hospital:—	
I.—Jaundice from Calculus impacted in the Common Duct; Dilatation of the Biliary Passages; Intumescence of the Liver; Death; <i>Post-mortem</i> Examination, - - -	209
II.—Transient Jaundice from Passage of Calculi; Inflammation of the Gall Bladder; Cystic Tumour, - - -	214
III.—Cases of Jaundice from Emotional Causes, - - -	217
IV.—Jaundice from Gastro-duodenal Catarrh, - - -	219
V.—Cirrhosis of the Liver; Death from Profuse Hæmatemesis,	219
ART. VII.—On Amputation of the Penis at the Pubes. By HENRY J. TYRRELL, F.R.C.S.I., M.R.I.A.; Surgeon to the Mater Misericordiæ Hospital, &c., - - - - -	222

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative. By SAMUEL D. GROSS, M.D., LL.D., D.C.L., Oxon., -	226
2. Current Literature of Insanity:—	
I.—Lectures on Madness in its Judicial, Legal, and Social Aspects. By EDGAR SHEPPARD, M.D., &c., &c., - -	253
II.—Insanity in its Relations to Crime: a Text and a Commen- tary. By WM. A. HAMMOND, M.D., &c., &c., - -	253
III.—American Journal of Insanity, 1872, - - -	253
3. A Treatise on Gout, Rheumatism, and Rheumatic Gout. By AUSTIN MELDON, &c., - - - - -	264



## PART III.—MEDICAL MISCELLANY.

	PAGE
Transactions of the Medical Society of the College of Physicians:—	
On Casella's Spirometer. By T. W. GRIMSHAW, M.D., - -	266
A Case of Cerebro-spinal Meningitis, in which Hypodermic Injections of Morphia and Atropia were freely used. By JAMES LITTLE, M.D., &c., - - - - -	266
A Case of Hysteria. By S. M. MACSWINEY, M.D., - -	271
Proceedings of the Dublin Obstetrical Society:—	
Fibrous Tumour of the Uterus. By G. H. KIDD, M.D., &c., -	281
Specimens of Mole Pregnancy and of so-called Uterine Hydatids. By T. MORE MADDEN, M.D., &c, - - - - -	281
A Case of Hysterical Convulsions, with some Remarks on Amenorrhœa. By F. T. PORTER, L.R.C.S.I., - - - - -	287
Rupture of the Urethra and Perinæum during Labour. By S. M. MACSWINEY, M.D., &c., - - - - -	290
Proceedings of the Pathological Society of Dublin:—	
DR. A. W. FOOT on Ulceration of the Ileum in Enteric Fever; Successive Hæmorrhages; Death on the Fifteenth Day, - -	296
DR. A. W. FOOT on Perforation of the Ileum in Enteric Fever; Extravasation of the Intestinal Contents, - - - - -	297
MR. TUFNELL on Aneurism of the Abdominal Aorta, - -	299
DR. NIXON on Enteric Fever; Peritonitis without Perforation of the Intestine, - - - - -	301
MR. THORNLEY STOKER on Deformity of the Head resulting from the Cicatrix of a Burn, - - - - -	302
DR. QUINLAN on Carcinoma of the Liver, - - - - -	303
Books Received, - - - - -	Cover

# PERIODICALS WITH WHICH THE DUBLIN JOURNAL . IS EXCHANGED.

## GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. The Half-yearly Abstract of the Medical Sciences. Churchill.
5. Pharmaceutical Journal. Churchill.
6. The Lancet.
7. The British Medical Journal.
8. The Asylum Journal of Mental Science. Churchill.
9. The Glasgow Medical Journal. Dunn and Wright.
10. The Athenæum.
11. The Dublin Medical Press.
12. The Westminster Review. Trübner.
13. Transactions of Obstetrical Society. London : Longmans.
14. The Practitioner ; a Monthly Journal of Therapeutics. Macmillan and Co.
15. The Journal of Anatomy and Physiology. Macmillan.
16. The British Journal of Homœopathy. London : Henry Turner and Co.
17. Irish Hospital Gazette. Dublin.
18. The Obstetrical Journal. London : J. and A. Churchill.

## INDIA.

19. Indian Medical Gazette. Calcutta : G. Wyman and Co.

## AUSTRALIA.

20. The Australian Medical Journal, Melbourne: Stillwell and Knight. London : H. Baillière.

## AMERICA.

21. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Henry C. Lea. London: Trübner and Co.
22. The American Journal of Science and Arts. Conducted by Professors B. Silliman, and J. D. Dana, &c. New Haven: Editors.
23. The American Journal of Insanity, Utica, N.Y. State Lunatic Asylum.
24. The American Journal of Obstetrics and Diseases of Women and Children, New York : W. A. Townsend and Adams. London : Trübner and Co.

## AMERICA.—continued.

25. Canada Medical Journal. Montreal : Dawson, Brothers.
26. The New York Medical Journal. New York and London : D. Appleton and Co.
27. The Medical and Surgical Reporter. Philadelphia : S. W. Butler, M.D.
28. The Richmond and Louisville Medical Journal. Louisville, Ky. : E S. Gaillard, M.D.
29. The Medical Record. New York : Wood & Co.
30. The American Practitioner. Louisville, Ky. : John P. Morton and Co. London : C. D. Cazenove.
31. The Philadelphia Medical Times. Philadelphia : J. B. Lippincott and Co.
32. The Sanitarian. A. S. Barnes and Co., 111, William-street, New York.
33. The American Chemist, School of Mines, Columbia College, East Forty-ninth-street, New York.

## FRANCE.

34. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue des nouvelles scientifiques, nationales et étrangères, &c. Paris: Labé.
35. Journal de Médecine de L'Ouest. Nantes: Mellinet.
36. Journal de Pharmacie et de Chimie, &c. Paris: Victor Masson.
37. L'Union Médicale. Paris: Bureau, Rue de la Grange. Batelière.
38. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
39. Archives Générales de Médecine. Paris: Asselin.
40. Bulletin de l'Académie Impériale de Médecine. Paris: Baillière.
41. Revue de Thérapeutique Médico-Chirurgicale. Paris: Dr. A. Martin-Lauzer.
42. Journal de Médecine et de Chirurgie Pratiques a l'Usage des Médecins. Par Lucas-Championnière. Paris.
43. Journal des Connaissances Médicales Pratiques. Paris: J. B. Baillière et Fils.
44. Annales Médico - Psychologiques. Par MM. Baillarger, Cerise, et Lunier. Paris: V. Masson.
45. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Par le Docteur Félix Bricheateau. Paris.

FRANCE.—*continued.*

46. Répertoire de Pharmacie. Par M. le Dr. Bouchardat. Paris: G. Baillière.
47. Gazette Médicale de Strasbourg.
48. Journal de Médecine de Bordeaux.
49. L'Union Médicale de la Gironde, Bordeaux.
50. Lyon Médical, Organe Officiel de la Société Impériale de Médecine. Lyon: Mégret.
51. Journal de Médecine Mentale. Par M. Delasiauve. Paris: Masson et Fils.
52. Archives de Médecine Navale. Paris: J. B. Baillière et Fils.
53. Revue Photographique des Hôpitaux de Paris. Paris: Adrien Delahaye.
54. Le Mouvement Médical. Paris: Rue des Ecoles.
55. La France Médicale. 21 Rue de la Monnaie, Paris.
56. Revue des Sciences Médicales en France et à l'étranger. Paris: G. Masson.
57. Le Progrès Médical. Paris: E. Duval.
58. Monsieur le Docteur Dechambre, Paris: 91, Rue de Lille.

## BELGIUM.

59. Bulletin de l'Académie Royale de Médecine de Belgique, Bruxelles.
60. Annales D'Oculistique. Bruxelles.
61. Annales et Bulletin de la Société de Médecine de Gand.

## GERMANY.

62. Vierteljahrsschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.
63. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.
64. Wochenblatt der Zeitschrift der k. Gesellschaft der Aerzte in Wien (Beilage zu den Jahrbüchern). Redigirt von A. Duchek, C. Langer, A. Schauenstein. Leipzig: Hinrichs.
65. Deutsches Archiv. für Klinische Medicin. Erlangen: Th. Blasings.
66. Jahrbuch für Kinderheilkunde und Physische Erziehung. Leipzig: B. G. Teubner.

## PRUSSIA.

67. Archiv für pathologische Anatomie und Physiologie, &c. Herausgegeben von R. Virchow. Berlin: G. Reimer.

PRUSSIA.—*continued.*

68. Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medicin. Herausgegeben von Damerow, Flemming, Roller; durch Heinrich Laehr. Berlin: Hirschwald.
69. Berliner Klinische Wochenschrift. Berlin: Hirschwald.
70. Archiv für Klinische Chirurgie. Herausgegeben von Dr. B. von Langenbeck. Berlin: Hirschwald.
71. Monatsschrift für Geburstkunde und Frauenkrankheiten. Berlin: Hirschwald.
72. Archiv für Psychiatrie und Nerven Krankheiten. Berlin: August Hirschwald.
73. Centralblatt für die medicinischen Wissenschaften. Berlin: August Hirschwald.

## HOLLAND.

74. Archiv für die Holländischen Beiträge zur Natur- und Heilkunde, Herausgegeben von F. C. Donders, Utrecht, und W. Berlin, Amsterdam, Utrecht: C. Van Der Post.

## NORWAY.

75. Norsk Magazin for Lægevidenskaben. Udgivet af det medicinske Selskab i Christiania. Redigeret af Schoenburg. E. Winge. Bidenkap. Christiania: Paa Th. Steens Forlag.

## SWEDEN.

76. Hygiea, Medicinsk och Farmaceutisk Maonads-skrift. Stockholm .P. A. Norstedt och Söners förlag.

## DENMARK.

77. Bibliothek for Læger. Udgivet af Direktionen for det Classenske Literaturselskab. Redigeret af Dr. K. Brüniche. Copenhagen: Reitzels.
78. Hospitals-Tidende. Optegnelser af praktisk Lægekunst fra Ind- og Udlandet. Copenhagen: Jacob Lund. London: Asher and Co.

## ITALY.

79. Bulletino delle Scienze Mediche. Pubblicato per cura della Società Medico-Chirurgica di Bologna.
80. Giornale Veneto di Scienze Mediche.
81. Lo Sperimentale Giornale Critico di Medicina e Chirurgia per servire ai Bisogni dell'Arte Salutare. Direttore Prof. C. C. M. Butalini. Florence.

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

MARCH 1, 1874.

---

### PART I.

### ORIGINAL COMMUNICATIONS.

---

ART. VI.—*Select Clinical Reports.* By ARTHUR WYNNE FOOT, M.D.; Junior Physician to the Meath Hospital.

- I.—JAUNDICE FROM CALCULUS IMPACTED IN THE COMMON DUCT; DILATATION OF THE BILIARY PASSAGES; INTUMESCENCE OF THE LIVER; DEATH; POST-MORTEM EXAMINATION.
- II.—TRANSIENT JAUNDICE FROM PASSAGE OF CALCULI; INFLAMMATION OF THE GALL-BLADDER; CYSTIC TUMOUR.
- III.—CASES OF JAUNDICE FROM EMOTIONAL CAUSES.
- IV.—JAUNDICE FROM GASTRO-DUODENAL CATARRH.
- V.—CIRRHOSIS OF THE LIVER; PROFUSE AND FATAL HÆMATEMESIS; POST-MORTEM EXAMINATION.
- I.—*Jaundice from Impaction of a Calculus in the Outlet of the Common Duct; Dilatation of the Biliary Passages; Intumescence of the Liver; Death; Post-mortem examination.*

AN unmarried gardener, aged sixty, was admitted into the medical wards, 28th February, 1872. He was a lean, gaunt, dark-complexioned man, above the middle height, weighing 123 lbs.; he was generally and deeply jaundiced. He complained of nausea both at and after his meals. The nausea was attended with pains in the epigastrium. These he was in the habit of relieving by making himself vomit, which he did by tickling the fauces. He



also suffered from burning heat in the interscapular region, and from a dull frequently-recurring pain, which used to set out from the right hypochondrium, and extend upwards to the epigastrium. His stomach was in the habit of swelling about an hour after meals, and this swelling was attended with pain, which, as well as the feeling of sickness, disappeared after he had induced vomiting. The hepatic region was sensitive to pressure; there was considerable increase of liver dulness below the tenth right rib, and laterally for three inches to the left of the middle line; no surface irregularity could be detected by the most careful palpation of the enlarged liver; there was no other abdominal tumour. He had been ill for six weeks before his admission, and persisted in maintaining that the precise cause of his condition was some indifferent whiskey which he had been induced to drink at a funeral at Glasnevin; he considered that it had burned his stomach, and frequently lamented his indiscretion in having swallowed it. He often asserted that he had always been quite healthy up to the date of the alleged cause of his illness. His history of the case lent support to a view that the original cause of his present condition might have been gastro-duodenitis leading to catarrhal obstruction of the biliary passages; however, there was now a complete absence of febrile symptoms; the pulse was 52, full and regular. The deep brown urine gave indications (with Pettenkofer's test) of the presence of the biliary acids, as well as of bile pigment; it was acid, sp. gr. 1023·5, and contained albumen in small quantity; its sediment presented some renal epithelium, blood corpuscles, and crystals of oxalate of calcium. Hedgehog crystals of tyrosine, and globular forms of leucine, were also found in it after the customary preparation. He slept heavily, and was generally drowsy. There was no itching of the skin.

The case was considered an unpromising one, and an electuary of confection of scammony and confection of sulphur was ordered to obviate his habitual constipation; this produced free evacuations, and on 5th March the jaundice had almost disappeared; but on the same day he had some "burning pain" in the *left* hypochondrium and epigastrium, relieved by the subcutaneous injection of morphia. 8th March.—He got a fresh attack of jaundice, with a return of biliary colic, not relieved by his own treatment—induction of vomiting. For this the morphia injection was repeated, and pills of opium ordered; pulse, 120; the rigidity of the recti abdominis was very marked. On the 14th the jaundice was again almost gone, the

hepatic enlargement remaining. He now continued free from marked jaundice for a considerable time, but constantly experienced uncomfortable sensations of dragging and tightness in the right side, even when free from actual pain. He said that he "felt his guts pulling on his body;" he felt "caught" in the hepatic region when he drew his breath fully or stretched himself. The emaciation was progressive and increasing; he observed that his food did him no good. The "aching" and "burning" sensations in the epigastrium were relieved best by opium; castor-oil kept his bowels regulated without pain; colocynth always griped him. 24th April.—Took "creeping chills in the flesh" over the abdomen and thighs last night; these were interpreted as rigors from dilatation of the biliary passages. He repeatedly observed that his "food don't digest."

Soon after this he got well enough to leave the hospital. He was able to do light work as a gardener through the summer; but, continuing to loose flesh, and having got so weak that he could hardly walk, he was re-admitted 7th November, 1872, at which time he had slight ascites. His feet had swelled on the 4th November; he could now only lie on the right side, for when he lay on the left he felt his "stomach casting over;" he gets a pain in the belly after eating, which pain continues for half an hour—it is "not a violent pain, but wears off, wind-like." He complained also of "the lump in his stomach," and of an aching weariness across the lower part of the abdomen, but has "no severe pain." No shoulder pain, the pain goes out to his back. A firm enlargement of the liver occupied the right hypochondrium and the epigastrium, to the level of a horizontal line through the umbilicus. Though thirsty, he cannot bear cold water; he could only take warm drinks; he had no marked dyspepsia, and can eat fat better than lean; his colour at this time was a tawny olive, with a brightly jaundiced eye. His urine, loaded with urates, purpurine, and bile pigment, contained a trace of albumen. 14th November.—Diarrhoea came on, and continued till the 17th. On the 22nd, though his condition appeared unchanged, he spoke to the men in the ward of his death, and of where he wished to be buried. To me he made no complaint or observation further than his invariable salute as I passed his bed in the morning, so that seeing him sitting up as usual—his thin yellow face surmounted by his conical, white, tufted night-cap—being a little pressed for time, I did not stop to speak to him. At 7 p.m., after passing a large motion, so fœtid that it sickened some patients in the ward, he died suddenly in his bed.

When the body came to be examined 17½ hours after death, the basal arteries of the brain (which weighed 48½ ozs.) were almost universally diseased, exhibiting nodular and annular white spots and thickenings; in the intra-cranial stage of the left vertebral artery was a shot-like concretion of fibrine, which could be moved a little backwards and forwards, but was otherwise stuck in the vessel. The process of the choroid plexus passing into the middle horn of the left lateral ventricle presented numerous vesicular dilatations, some as large as good white currants, and of a warm whitish tint. A certain amount of central softening of the fornix, and some opacity of the arachnoid covering the right hemisphere, were the only other noticeable changes in the cranial cavity. The heart presented some bilious œdema of the sub-epicardial tissue; it was free from fatty degeneration; the valves were all normal. The aorta—whose lining membrane was deeply jaundiced, and whose trunk and branches, as far as Poupart's ligament, were extensively atheromatous and here and there calcified—was occupied by a firm coagulum, prolongations of which passed up each of the vessels springing from the arch. The right ventricle was filled with a jaundiced clot. The lower extremities and genitals of the saffron-coloured corpse were œdematous; the abdomen bore lines of nitrate of silver marking the dimensions of the liver as ascertained by percussion during life; the peritoneal cavity contained about a pint of yellowish serum; numerous stringy adhesions appeared between the enlarged liver and the right side of the abdominal wall and right half of the diaphragm on the one side, and on the other between its left lobe and the stomach and spleen; these latter adhesions lay superficial to the gastro-hepatic omentum. The prominent occupation of the epigastrium by the enlarged liver was very noticeable. The distended gall bladder, greyish-white in colour, projected much below the free edge of the liver, measuring four and a half inches long, and two and a half broad at its fundus; the dilated cystic duct measured five lines, and the common duct one and a quarter inches in breadth. The surface of the liver, where not roughened by adhesions, was smooth, and mottled with yellowish dots and spots, which gave it a marbled appearance; these were minute, superficial, distended bile ducts. On the convex surface of the right lobe were three vertical sulci, made by the pressure on the organ of folds of the diaphragm in expiration. The gland measured twelve and a half inches in breadth, from right to left; the right lobe was eight and a

half in vertical measurement by seven and a quarter inches in breadth, the left six inches in height by five and a quarter inches in breadth. A hard, round lump, like a bullet, could be felt in the duodenum, which, when opened, showed the obstructed orifice of the common duct projecting into the bowel, not unlike the cervix of a small uterus; two blackened holes on the summit of the projection indicated that the calculus might before long have delivered itself by a sloughing process; a probe, passed along the pancreatic duct, sounded the stone; the pancreatic duct was distended with bile, which had regurgitated from the common terminal orifice, though the gland itself was but little stained; a probe pushed from the duodenum against the stone, where it pressed down against the distended end of the common duct, moved it back, and permitted an immediate flow of brown bile from the duct through the two sloughy apertures, but when the probe was removed the gush of bile jammed down the stone again, and replaced the obstruction. The stone was barrel-shaped, smooth on all sides, and fitted the common duct just like a plug. The liver, when incised, gave exit to streams of bile, and displayed numerous small biliary cysts, like abscesses, the result of over-distension of expanded bile ducts. The calculus was 2 inches in one circumference, and  $2\frac{3}{20}$  inches in the other; its diameters were  $\frac{1}{20}$ ths inch in one direction, and  $\frac{1}{20}$ ths in the other; it was about as large as an ordinary cherry, and was a solitary concretion. Section of it through the middle did not bring to light any *nucleus*; the cut surfaces showed a glossy, dark-greenish-bronze colour, like that of the tail feathers of a cock. The *shell* had no concentric or radiating arrangement; the thin *crust* was light brown and friable.

Dr. S. H. Webb kindly made a most accurate analysis of half of the calculus. 100 parts contained:—

Moisture,	-	-	-	-	-	4.50
Matters soluble in water—viz., alkaline glyco-						
cholates and taurocholates, and colouring						
matter (bilifuscin),	-	-	-	-	-	10.32
Cholesterin,	-	-	-	-	-	2.94
Fats and traces of colouring matter soluble in						
alcohol (biliverdin and bilirubin),	-	-	-	-	-	23.61
Bile resin, &c.,	-	-	-	-	-	51.33
Calcareous compounds,	-	-	-	-	-	7.30
						<hr/>
						100.00



From the analysis it appears that it was a resinous calculus, very deficient in cholesterin—which usually forms the principal ingredient in the ordinary forms of gall-stones, many of them being composed entirely of it. It may be reckoned that gall-stones contain, on an average, from 70 to 80 per cent. of this substance.<sup>a</sup> From the large quantity of bile resin, it more nearly resembled a bezoar than an ordinary gall-stone.

This case ran its course in ten months. The *smooth* enlargement of the liver with recurring jaundice indicated intumescence of the liver with bile from some obstruction of an intermitting nature. In the other usual *smooth* enlargements—amyloid and fatty liver—jaundice, if present at all, is generally both slight and permanent. The integrity of the heart excluded the well-known swelling of the liver with jaundice observed in certain forms of cardiac disease. The frequent colics shortly after food, when duodenal digestion ought to be going on, indicated gall-stones in the common duct, and were, no doubt, due to the efforts of the bile to get into the duodenum, in response to the stimulus of food pressing the sensitive orifice of the common duct. The diagnosis lay between one stone in the common duct of an angular form, and several passing in succession. The absence of acute pain was puzzling, for in answer to frequent inquiries on this point, he used to say he had “no severe pain,” that the burning sensations he had “were not violent pain.” The opium was used because it relieved the distressing internal sensations of heat and soreness. The *post-mortem* examination explained the absence of the usual agony caused by gall-stones. This stone was too large to attempt the narrow defile leading into the duodenum, and by damming up the bile it kept itself from being tightly grasped by the common duct, distended throughout by its fluid contents. Twice during his first stay in hospital, the jaundice quickly disappeared in a remarkable manner, which in this case could only be ascribed to some fortunate shifting of the stone. In this point the case contrasted instructively with the persistent icterus in a case of fatal obstruction of the common duct, by a papillomatous tumour, in the medical wards during the same session.<sup>b</sup>

## II.—*Transient Jaundice from the passage of Calculi; Inflammation of the Gall-bladder; Cystic Tumour.*

A woman of forty-three years of age, mother of twelve children, of whom but three were living, was admitted into the medical wards

<sup>a</sup> Frerich's Dis. Liver, II., 492.

<sup>b</sup> Irish Hospital Gazette, Jan. 1, 1874, p. 1.

with a sudden attack of great pain and tenderness in the right hypochondrium, retention of urine for two days, constipation, dry retching, and disinclination to take food or drink. Her respiration was shallow; she was unable to take a full breath. There was no disease of the respiratory system; she had frequent rigors, in which her "teeth chattered;" these chills were followed by heat and perspiration. She had pain in the right acromion and at the back of the right scapula; there was a greenish-yellow tint in the conjunctiva; she could rest in no position but on her back; she had no fever, but felt very queer, confused in the head, and did not know what was over her. She was a washerwoman, and immediately before this attack had exerted herself greatly over "a large boiling;" she thought she might have got cold after it. A smooth painful tumour was discovered in the region of the gall-bladder, reaching nearly to the umbilicus. Three leeches were applied over the tumour, and bleeding from the bites promoted by hot poultices. This gave "the greatest relief" to the pain; an enema of water, as hot as could be borne, with epsom salts and castor-oil, acted on the bowels, and made her pass urine, very dark-coloured, immediately afterwards.

It was ascertained that she had had an attack, similar to the present one, eight months previously, that it had lasted three weeks, and was unaccompanied with jaundice. On further inquiry it was found that in the last fourteen years she had more than twenty attacks of severe and sudden hypochondriac pain. Of these three had been much worse than the rest. The "lump," as she called the tumour now present, had appeared after several of them, as a "hard stony lump like an orange," and then disappeared until the next attack. She had always been "bilious," and the attacks of pain were generally induced by her being "irritated or fretted." Shortly before this attack her husband had drowned himself, and soon after she left hospital she found her son endeavouring to hang himself behind a door. After the evacuation of the bowels and bladder, and the relief of the local pain, she felt her head quite right, and lost the confused drowsy feel she had when admitted. Her pulse was 86; tongue moist, with a brownish fur; she had "a disgusting taste in the mouth," which, when pressed for a description, she said was "nuisance-like"—probably faecal. It could now be ascertained that, without any unusual increase in the size of the liver, there was in the hepatic region an oval tumour, the size of a duck-egg, over which the abdominal tenderness was concentrated. She preferred

to have it handled by herself than by others; she described it as "long at one end," and said it "sometimes got longer." More leeches and poultices were applied to it, with the effect of so much reducing the tenderness that the tumour could be manipulated and gently percussed. She observed that in the commencement of this attack she got relief by holding "the lump" between her fingers. The jaundice did not increase so as to stain the skin perceptibly; she was naturally of a dark sallow complexion; the urine and the conjunctivæ alone indicated the presence of bile pigment, and from these it disappeared quickly. The disagreeable taste in the mouth continued longer; there was no itching of the skin. On the eighth day after the commencement of the attack, the tumour was almost imperceptible, and the conjunctivæ had regained their natural colour. She could not yet lie on her left side, because when doing so she felt "a great fall come within" her, and she got no ease till she turned on her back. On the tenth day the urine was perfectly clear; she got up and ate meat with a relish. There still remained a soreness in the region of the gall-bladder, and some weakness in her back. She left hospital in a few days.

In this case no calculus was found in the evacuations, although a pretty close look-out was kept. Although their discovery is a very satisfactory confirmation of diagnosis, no one acquainted with the practical difficulties of such research will consider it an always necessary proof. In the early stage of the attack she derived relief from squeezing the gall-bladder, distended by the accumulated bile; afterwards, when tenderness became acute from the lining membrane of the gall-bladder and bile ducts having become inflamed, it became almost impossible to touch that region. In the early stages of obstruction pressure may even be useful. Sir Thomas Watson\* mentions that one of his patients, who voided fifty-five calculi in the space of five weeks, began to pass them four or five at a time, with paroxysms of severe pain, just after having had the hepatic region diligently shampooed and kneaded as he lay on his left side in a warm bath, so that they seemed to have been mechanically pressed out of the gall-bladder and through the ducts.

The nature of this case appeared to be that this woman possessed a gall-bladder furnished with a stock of calculi which escaped from time to time, the disturbing cause in this last attack being unusual muscular exertion. Previous ones, being smaller, had escaped

\* Practice of Physic. II., 680.

without jaundice, not having sufficiently obstructed the orifice of the common duct to cause any great retention of bile. On this occasion a larger stone obstructs the exit of bile from the common duct; the retention first and principally distends the gall-bladder, which, as the natural receptacle, yields to the pressure much sooner than the hepatic ramifications—so cystic tumour occurred without general intumescence of the liver, as in the former case. Then inflammation attacks a gall-bladder already irritated by locomotive calculi, and on the escape of the stone the disturbance subsides till the next occasion.

### III.—*Cases of Jaundice from Emotional Causes.*

A small-sized, red-complexioned man, aged twenty-six, not of very temperate habits, got into a great passion in a sparring match; he felt ill immediately afterwards, but had neither rigors nor severe pain; his urine became jaundiced at the time, his eyes subsequently; the first place he noticed the skin to be yellow was on the integument of the prepuce. He was admitted into hospital two months after the commencement of the attack, generally and strikingly jaundiced, about the colour of a good Belgian canary, but quite free from fever; the liver was enlarged vertically and transversely, and was very tender; he winced and made an expiration at the slightest touch over the hepatic region; there was also a “fulness” in the epigastrium; a “burning” sensation in the stomach, as well as “horrid”-tasting eructations, with lassitude, heaviness, and occasional dizziness. The fæces were clay-coloured; there was frequent itching of the skin of the trunk at night, and his body was covered with the marks of his scratchings. His appetite was fair, and he was able to be up and about, but was anxious to get cured of his “yellowness.” He was kept on low diet and purgatives. The day after admission he was given a pill containing cal. gr. iij., res. scam. gr. iv., res. podophylli gr. j., and each subsequent day one containing ext. aloes gr. ij., res. scam. gr. iij., res. podophylli, gr.  $\frac{1}{2}$ . He had been admitted 28th April, 1873. May 3rd.—Eyes and urine much less jaundiced; fæces still abnormally pale. 5th.—Skin now a light primrose tint; hepatic tenderness much lessened; dulness now only for two fingers-breadth below the ribs in the mammary line, and leftwards, not reaching the linea alba. He went out, 7th May, with a tinge remaining in the conjunctivæ and skin, but he said he had not felt so light and well for six months. He re-appeared 10th June,



1873, complaining again of pain in the liver and continual itching of the skin, which had not regained its proper colour since the late attack, constipation, and pain in the temples; the conjunctivæ were tinged; the symptoms disappeared after the use of a mixture of sulphate of magnesia in co. dec. aloes.

A strong and active young man, aged twenty-nine, who had never been a day sick in his life, was admitted, 29th April, 1871, very deeply jaundiced. He had latterly suffered much annoyance and persecution in consequence of having changed his religion. After feeling unwell for two or three weeks his eyes became yellow on the 23rd. His skin was universally itchy, but more so along the right side, and especially at night; a bitter taste, "like aloes," in his mouth was imparted to everything he made use of, even water. He was not confined to bed; there was no local pain or tumefaction perceptible; he was drowsy and sleepy. He was treated with compresses over the liver, wetted with equal parts of dilute muriatic acid and water. These soon brought out a bright-red, lichenoid eruption. He was also ordered ammon. hydrochlor.  $\zeta$ ij., suc. tarax  $\zeta$ ij., ac. hydrochl. dil.  $\zeta$ ij., aq. ad  $\text{3vii}$ .; a tablespoonful three times a day. The constipation was overcome by boluses of calomel and jalap. May 15th.—He had nothing to complain of but the deep yellow colour of his skin, which persisted in a marked degree. He soon after left the hospital to take a course of warm salt-water baths, and ultimately quite lost all traces of jaundice.

A thin, delicate-looking milliner, aged eighteen, was admitted for jaundice, 13th October, 1871. About three weeks previously she had got, as she thought, a severe cold, and soon afterwards she felt "a lump" in the epigastrium, and pains in the right hypochondrium, right shoulder, and from that across to the opposite shoulder, with a failure of appetite and vomiting of liquid foods, such as beef-tea and new-milk. The fæces soon lost their natural colour, and the urine became deeply tinged with bile. Before she had any external appearance of jaundice she used to observe to her friends that white things about her appeared yellowish. Her illness was attributed to the irritation caused her by a young lady employed in the same establishment, who was a source of much annoyance to her, making her "frequently cry with vexation." This unpleasantness was consummated by her friend having secured the affections of a young man whom the patient had regarded as solely devoted to her. She soon got well under the

use of aperients and dilute muriatic acid. She was very apprehensive about the restoration of her naturally fair skin to its normal appearance, but her fears were not realized, as she was fortunate in getting rid of the bilious tinge in a shorter period than usual. The benzoate of ammonia seemed to accelerate the departure of the icteric stain.

#### IV.—*Jaundice from Gastro-duodenal Catarrh.*

A mixer of colours in a druggist's establishment, aged forty-two, was admitted, 9th October, 1872, very deeply jaundiced. He had been feeling unwell for three weeks, but was first observed to be "yellow" six days before admission. There were no symptoms of gall-stones; he had had acid vomiting before admission; his habits were not, in respect of drink, those of a very temperate person; he had been of late much depressed by the loss of his wife; he had no hepatic pain, only "a fulness and soreness" in the epigastrium; he was very itchy, especially at night when he got warm in bed; a "bad smelling wind" ("bad eggs") came off his stomach; he could not taste his food properly; his clay-coloured stools were unusually foetid; he felt very heavy in himself; there was very slight enlargement, and that smooth, of the liver. He was principally distressed by the foetid eructations. After the administration of a bolus containing calomel gr. x., res. podophylli gr.  $\frac{1}{4}$ , the use of the following mixture removed the foul air and the painful distension of the epigastrium:—Co. tr. lav., co. tr. card., aa.  $\mathfrak{zss.}$ , spt. cajeputi,  $\mathfrak{zss.}$ , syr. zingib,  $\mathfrak{zss.}$  aq. m. pip. ad  $\mathfrak{z}$ . viij., s.  $\mathfrak{zj}$ . t. d. On 14th he asked to be allowed up and to have meat. The jaundice was remarkably diminished by free purgation; each morning after the administration of a bolus of calomel and jalap, or calomel and podophyllum, the colour was obviously lessened. He left hospital, 25th October, relieved of all the constitutional symptoms, though still yellow. He was admitted again, 7th July, 1873, not for jaundice, of which he bore no traces, but for vertigo dyspeptica, of which he was soon relieved under the use of bicarb. potassæ  $\mathfrak{zj}$ ., tr. nuc. vom.  $\mathfrak{zij}$ ., inf. quassia ad.  $\mathfrak{zviij}$ .,  $\mathfrak{zi}$ . t. d.

#### V.—*Cirrhosis of the Liver—Death from Profuse Hæmatemesis.*

A full-sized, powerfully-made man, aged forty, was admitted into the medical wards of the Meath Hospital, at noon, on Wednesday, 12th July, 1871. He sought admission on account of a

recently-observed dropsy in the belly and legs. Though a hard drinker, he said he had enjoyed good health. The dorsum of the glans penis presented a very extensive cicatricial depression; there were also cicatrices in each groin. He had no jaundice. The evening before his admission he went on a car from his home to the house of a gentleman to get an order to the hospital; he was unusually sensitive to the jolting of the car; he felt as if it would "tear his insides," and he had to hold on tight. The jolting brought on a slight "spitting of blood," for the first time. He got the order, and on the following day he set out in a cart for the hospital, a distance of eighteen miles. He reached it at 4 p.m., and shortly after admission began to vomit dark, greyish-black fluid, in colour like mushroom-sauce. I saw him at 7 p.m., when he was faint and weak, with an irregular pulse of 120; there were some red coagula in the blackish messes he had vomited. He was ordered 15 gr. doses of gallic acid at short intervals, and 30 min. doses of tincture of the perchloride of iron in iced water occasionally, with ice as often as he could be got to swallow it. The hæmatemesis continued with short intermissions till 3 a.m. the following morning (13th). By that time the pans of two night-chairs were completely filled with the vomited matters. He also passed blood from the bowels during the night. His answer next morning, to an inquiry as to how he felt, was—"A hundred pounds better." He was quite pleased with the subsidence of the ascites, which was very considerably reduced. There was no tenderness or pain anywhere over the abdomen. Some distended veins were noticeable, but they were few and very moderately enlarged. He was ordered to continue the gallic acid, but in reduced doses, and was able to take milk and beef-tea. Before 6 p.m. the hæmatemesis returned, and when I saw him at that hour a night-chair pan had been almost filled with the same sepia-coloured, catsup-like stuff; pallor had crept over his face; the pulse was 130, weaker than before; the temperature low; the pupils wide and very sluggish; muscular relaxation; the contractions of the heart very feeble; a spanæmic bruit in the left carotid artery. Jactitation was commencing, the respiration was sighing; the lung sounds clear and loud, without rale. Whenever he rallied at all it was to vomit. He died in a syncopæal state at 4 a.m. the following morning, thirty-six hours after his admission.

The corpse, notwithstanding the death from hæmorrhage, presented a very obvious amount of hypostatic congestion, except

where the pressure of the heavy body had displaced the blood from the flattened parts in contact with the table. The peritoneal cavity contained a large quantity of clear, straw-coloured serum. The stomach was largely distended with gases, and contained some fluid similar to that which had been vomited, and with which the nose and mouth of the body were still discoloured. There was no abrasion or ulceration of the mucous membrane of the stomach discernible over its slate-coloured surface.

The liver afforded a marked specimen of cirrhosis; it was contracted, hard, and of a pale buff-grey colour; its uneven surface presented milky opacities of the peritoneum in the depressions between the nodules with which it was studded. Firm and gristly to cut, its section exhibited differently tinted lobules, many of them ochreous; amid the paler interlobular connective tissue, these ochreous lobules could be easily isolated and picked out; the organ was exsanguine; its ligaments showed dilated but empty anastomotic vessels. The spigelian lobe was especially shrivelled and nodulated, presenting most of the ochreous lobules. The larger branches of the portal vein were obviously increased in the thickness of their coats. The œdematous gall-bladder was the shape and colour of an egg; after the division of its connexions with the duodenum had allowed the peri-cystic connective tissue to be drained, the colour of its contents could be seen through the coats, now of ordinary thickness; five small, black, friable concretions of inspissated bile were found in its fundus among the thin orange brown bile. The purple-coloured spleen was not large, but probably had been much drained by the hæmorrhage; the kidneys, normal in size, shape, and consistence, were paler than usual. Different parts of the small intestines were stuffed with semi-digested blood, and the dark slatey colour of these parts contrasted strongly with the anæmic pallor of other parts of the canal. The heart was under the average size, and had much external fat over its right half. On different portions of the interior of the left ventricle and on its muscoli papillares there were sub-endocardial extravasations and ecchymoses. The aortic valves and the origin of the aorta were deeply stained, of a vermilion colour; the *post-mortem* examination was made eight hours after death. This coloration appeared to have been from imbibition of the pigment of the blood, as the appearance underwent such change in the course of five hours, although the parts were carefully covered up, that it was useless to have them drawn, as I had intended. There was a notable tendency to rapid putrefaction



observed in all parts of the body; the weather, at the time, was not at all remarkable for its warmth.

Death by hæmatemesis is very unusual in cirrhosis of the liver. Dr. Gordon, in 1854 (*Dub. Quart. Jour.*, Vol. XVII., p. 345), published a case of this event in cirrhosis, the more remarkable as it occurred in a boy of 15; he used to suffer from purpura and epistaxis; this boy had neither jaundice or ascites. Dr. Gordon quotes another case of this event, and mentions that Dr. Law, in 1829, drew attention to the occurrence, and held that it was the only disease of the liver in which hæmatemesis occurs. Among many cases of various forms of cirrhosis of the liver, including the syphilitic and the fibro-amyloid, I had not seen this mode of death until the present case. The recorded instances, not at all numerous, of this complication, show that there may or may not be ascites and abdominal varix, as fatal hæmatemesis has occurred with and without these phenomena. Murchison<sup>a</sup> mentions two cases of it, in one of which there was neither ascites nor enlargement of the abdominal veins, while in the other the ascites was copious, and the venous development unusually great. The hæmorrhage is undoubtedly an exhalation or diapedesis of blood from over-distended capillaries, and independent of any ulceration or macroscopic lesion.

ART. VII.—*On Amputation of the Penis at the Pubes.* By HENRY J. TYRRELL, F.R.C.S.I., M.R.I.A.; Surgeon to the Mater Misericordiæ Hospital, &c.

IN amputation of the penis, when, from the extent of the disease, it becomes necessary to remove the organ from the pubes, the crura are liable to retract beneath the pubic arch, and, consequently, much difficulty is often experienced in arresting the hæmorrhage from the stump.

An old man, aged seventy, with very extensive epithelioma of the penis, of four years' growth, was admitted into the Mater Misericordiæ Hospital, under my care, last November. The disease engaged so much of the organ that I was obliged to remove it as far back as the pubes, and I adopted the following simple method of preventing the contraction of the corpora-cavernosa.

Having with my left hand drawn the penis well forward, I trans-

<sup>a</sup> Lectures on Diseases of the Liver, p. 266.

fixed, with a strong acupressure pin, the crura immediately in front of the pubes, and tied a thin tape tightly round the penis behind the pin. I then, with one stroke of a small amputating-knife, cut through all the organ in front of the pin. The tape being gradually loosened, all the arteries requiring notice were twisted; the urethra was next slit for half an inch, and I united the mucous membrane of the urethra to the skin by three sutures of carbolized gut; a piece of cork was stuck on the sharp end of the acupressure pin, *which was not removed*. No dressing was applied; and the patient was directed, when he desired to pass water, to press the bell-end of a vaginal glass speculum against the pubes, and to micturate through it. I also desired my dresser, Mr. Dempsey, if any hæmorrhage came on during the day, to apply a figure of 8 suture round the pin and over the stump sufficiently tight to control it.

Everything went on well; no pain was caused by the pin. I did not remove it for three days. No further details are necessary.

It is quite evident that by the use of the pin in the manner described, *all danger of hæmorrhage is avoided*; and also, by leaving it in for a few days, rest to the stump is secured, and *time is given for the speedy union of the mucous membrane and integument*.

As far as I know, this mode of treatment has not been before described.

Erichsen,<sup>a</sup> when speaking on this subject, says:—"There are usually five arteries requiring ligature. In securing these trouble is not unfrequently experienced in consequence of the retraction of the stump that is left. The liability to this, however, is lessened by tying a tape round the root of the penis before the operation is commenced, or by directing the assistant to hold it firmly between the fingers, and thus to prevent the retraction of the stump, which may otherwise draw back and be buried under the pubic arch, almost in the perineum. Should it not do so, and oozing continue, a female catheter must be passed into the bladder, and a firm compress applied with a T bandage."

Gross,<sup>b</sup> in his "System of Surgery," p. 873, makes the following remarks:—"When the bleeding from the cavernous bodies is, as sometimes happens, unusually troublesome, the best way to arrest it is, to transfix them with an acupressure kneedle tightened by a ligature passed round it elliptically, as in the common hare-lip. The instrument is removed at the end of twenty-four hours.

<sup>a</sup> The Science and Art of Surgery. By J. E. Erichsen. Fourth Edition.

<sup>b</sup> A System of Surgery, &c. By S. D. Gross. Fifth Edition.

Bryant<sup>a</sup> makes no allusion to the retraction of the crura. He uses the galvanic cautery for the amputation of the penis, but when it cannot be had he recommends the old operation with the knife, and recommends Clover's clamp for compressing the penis instead of tape.

Humphrey,<sup>b</sup> in Holmes' "System of Surgery," Vol. I., p. 181, makes no mention of the matter at all; neither does Millar,<sup>c</sup> Skey,<sup>d</sup> Fergusson,<sup>e</sup> Syme,<sup>f</sup> Pirrie, or Nélaton.

Velveau, in his "Operative Surgery," English edition, p. 775, makes the following remarks when discussing the mode of treating hæmorrhage after this operation:—"The noose of thread passed through the corpora-cavernosa, as recommended by M. Langenbeck (Rust's Handb. der Chir., Vol. I., p. 167), would only be useful in cases where, as in that by M. Hall (Gaz. Med., 1836, p. 748), we were obliged to carry the bistoury as far down as under the pubes, and which, according to M. Rorbye, he had heard recommended in Germany."

Dr. Humphrey, in the article above quoted, says it was Mr. Teale<sup>g</sup> who introduced the method of keeping the orifice of the urethra open by slitting the urethra and uniting the mucous membrane to the skin; however, in this improvement, Velveau anticipated Teale, for, in his "Operative Surgery," p. 775, he recommends the uniting of the skin to the urethra by means of three points of suture, according to the process he had pointed out in speaking of cancer of the lips.

In Cooper's "Surgical Dictionary," second edition, edited by Lane, it is stated:—"The bleeding arteries are to be immediately ligatured. Should a general oozing from the wound still continue, it may usually be arrested by the application of ice to the part, or of lint dipped in ice-cold water; but should this be ineffectual, a styptic, such as the solution of the perchloride of iron, may be applied to the bleeding surface."

Chelius<sup>h</sup> speaks of the difficulty of arresting the hæmorrhage, and

<sup>a</sup> The Practice of Surgery. A Manual. By Thomas Bryant. 1872.

<sup>b</sup> A System of Surgery, &c. By various authors. Edited by T. Holmes. Second edition.

<sup>c</sup> The Practice of Surgery. By James Millar. Edinburgh. 1852.

<sup>d</sup> The Principles and Practice of Operative Surgery. By Frederic C. Skey. 1858.

<sup>e</sup> A System of Practical Surgery. By W. Fergusson. Fourth edition. 1857.

<sup>f</sup> Principles of Surgery. By James Syme. London. 1852.

<sup>g</sup> Med. Times and Gazette, Vol. XIX., p. 354.

<sup>h</sup> A System of Surgery. By J. M. Chelius. Translated by J. T. South. London. 1847.

mentions the means adopted by various operators:—"In removing the penis near the pubic bones, Schreger has recommended the cut to be made with repeated strokes to prevent the retraction of the stump, and render the application of the ligatures more easy. An assistant presses up the bulb from the perineum forwards towards the pubic angle, and then, the operator having first drawn the penis and the skin forwards, divides the skin upon the dorsum penis and ties the dorsal arteries. After making the second cut he proceeds in the same way with the cavernous arteries of the penis; and after the third, with the cavernous arteries of the urethra, and the bleeding having been thus stanch'd, the remainder of the penis must be cut through."

Langenbeck's method to prevent the retraction of the penis is, "to cut through the dorsum penis so deeply into the cavernous bodies, that he can see their white edges and the septum; a loop is then drawn through both, and the penis completely cut through. The ligature serves to keep the stump steady, and to draw it forwards."

The penis has also been removed by tying. Chelius:—"A silver male catheter must be passed through the urethra into the bladder, a waxen silk thread applied beyond the diseased and upon the healthy part of the penis, and introduced into a loop-tier or some particular instrument for tying a ligature. This instrument must be screwed so tight that the part before the ligature shall be deprived of all feeling; the instrument is to be fastened with sticking-plaster, and the cancerous part covered with lint and compressed. On the second or third day, usually, the largest portion of the dead penis may be removed with scissors or bistoury, without bleeding or pain, and on the fourth or fifth day the ligature separates."

The advocates of the elastic ligature (the use of which I believe to be barbarous, cruel, unscientific, and worthy of the dark ages of surgery) will, no doubt, apply it to the amputation of the penis, and assert that it is not only the safest, but the most *speedy* and effectual mode of proceeding.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

*A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL D. GROSS, M.D., LL.D., D.C.L., Oxon., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c., &c. Illustrated by upwards of fourteen hundred engravings. Fifth Edition, Greatly Enlarged and Thoroughly Revised. In Two Volumes. Philadelphia: Henry C. Lea. 1872.

[Concluded from p. 44.]

Mr. Gross gives statistics of tracheotomy in membranous croup, an operation which, he states, was fatal in the great majority of cases in which it was performed in the United States. He attributes this mortality to procrastinated operations.

“Of 783 cases that have occurred at the Hôpital des Enfants at Paris, 191, or 25 per cent., are said to have been successful. The subjects in all these cases had been ill for several days, and were affected with great difficulty of inspiration. Guérant, who has furnished these statistics, states that he has performed this operation 156 times in private, and saved 28 children, or 1 in 5½. Of 141 cases analysed by Martini, 66 were cured, and 75 died. As an off-set against these results, it may be added that of 351 cases of tracheotomy, performed on account of this disease, by twenty-one French surgeons, including a number of the most distinguished operators of Paris, 312 terminated fatally, affording thus a ratio of 8 deaths to 1 recovery. One of these surgeons operated forty times, and lost every one of his patients. Dr. A. Jacobi, of New York, in 1868, published the results of 213 cases of tracheotomy, performed by himself, Voss, Krackowizer, and Von Roth, of which 50 or 23½ per cent. recovered.”

With regard to the operation of thyrotomy, we are almost inclined to agree with Mr. Gross's estimate of its value:—

"The objections to this operation," he writes, "are the risk of injuring the vocal cords, and the occurrence of tracheitis, bronchitis, and pneumonitis, as immediate effects, and the remote risks of serious disease of the laryngeal cartilages, and the mucous membrane of the larynx, besides experience has shown that the operation does not insure immunity against repullulation."

Bryant, we should state, mentions in his "*Surgery*," that both he and Mr. Durham have laid open the larynx in the middle line, and removed warty growths from the glottis and epiglottis with complete success.

The very serious nature of fracture of the larynx will be seen from the following analysis by Mr. Gross:—

"Of the 27 cases analysed by Dr. Hunt, 17 died. In 8 laryngotomy was performed, with 2 deaths and 6 recoveries. More extended observations show even a greater mortality. Thus, of 62 cases collected by Hénocque and Durham, 50 died, and 12 got well, tracheotomy having been performed in 8. In every instance, 21 in number, in which the cricoid cartilage was fractured, the result was fatal."

Mr. Gross mentions some remarkable cases, to show the length of time foreign bodies may remain in the air passages. In one of these cases four artificial teeth, connected together by metal, were "inhaled." In this instance the substance was retained for thirteen years, and was found, on dissection, in the right thoracic cavity, into which it had passed by ulceration."

Sometimes foreign bodies are spontaneously expelled from the air passages. The time at which the expulsion occurs varies "from a few hours to many years." In a case reported to Mr. Gross by Professor Flint, "nearly three years elapsed;" and in another, mentioned to him by Dr. Wulkupf, the interval was "upwards of eleven years."

Of the deceptive truce that sometimes occurs when foreign bodies are in the trachea every surgeon should be aware. In a case recorded by Muys, a child six or seven years old, whilst playing, swallowed a bean, which caused great difficulty of breathing and inexpressible distress. Several useless attempts were made, under the notion that the bean was in the œsophagus, to get it up, or to push it down into the stomach; it fell lower in the trachea, and the symptoms ceased. The child no longer coughed, and he breathed

without difficulty; indeed, he suffered no inconvenience. Two or three days afterwards he went to play with some children, and in stooping forward was attacked with the same symptoms as at the moment of the accident; by rest they became quieted, but motion renewed them; at length, after several relapses, the child died in the third week.

A somewhat similar case occurred in the practice of M. de la Romiguiere, in the year 1748. In it a bean passed into the trachea. Between the day of the accident and the fatal result, the child was several times in the street playing with its companions. It lived till the ninth day. The bean was found in the trachea after death.

In the *Ephemerides of Natural Curiosities* will be found an account of the following additional and striking example of this deceptive truce:—

The oldest of the monks in the Abbey of St. Martin, near Treves, walking one day in the garden, could not resist the temptation of a fine cherry; he bent down the branch and took the fruit in his mouth. Having separated the stone with his teeth, he tried to swallow the whole in a hurry, as he heard the bell ringing for church; the stone slipped into the trachea, and immediately excited a most violent cough and retching, which the monk thought would have killed him. After this violent agitation he fell asleep for some hours, and felt nothing whatever of the accident for a whole year. At the end of that time he was attacked with cough and fever. These symptoms became more and more threatening, until at length he spit up a stone as large as a nutmeg; this was formed exteriorly of calcareous matter surrounding a nucleus formed by the cherry-stone. A copious purulent expectoration followed the expulsion of the foreign body, and the patient died some days afterwards.

An interesting point in the case of the four artificial teeth mentioned by Mr. Gross he has omitted to notice—namely, where it was supposed they were lodged previous to entering the thoracic cavity. It is impossible to imagine that they could have passed along the larynx and trachea.

No one would suppose that in the following case, recorded by Hechstetter, the foreign body could have been in the trachea, although it may have been impacted between the glottis and rima.

A citizen of Augsburg, under the notion of curing a pain in his teeth, put a Portugal ducat in his mouth. On waking he found the piece gone. As he swallowed easily, however, and moved his

tongue freely, he thought he must have swallowed the money; but some time after, finding himself becoming hoarse and getting thin, he consulted several physicians and surgeons, who could not reach the foreign body with their fingers, nor with any instrument. At the end of two years and two months he brought up the ducat again.

Probably Mr. Gross includes in the term air passages, the mouth, pharynx, larynx, trachea, and bronchi. We think, however, it would be better, when the inspiration of foreign bodies is in question, to limit this term to the larynx, trachea, and bronchi, the inspiration of foreign bodies usually commencing at the glottis, owing to some momentary abnormality in the process of deglutition, or of respiration.

Whether or not the following modification, by Dr. Benjamin Howard, of Silvester's method of performing artificial respiration, is an improvement upon the latter remains to be tested by experience. At all events, Dr. Howard's essay on the "Treatment of Persons Apparently Found Dead from Suffocation," was awarded a prize by the American Medical Association, in 1871.

Dr. Howard's plan is carried out as follows—"After having cleared away all mechanical obstruction, the patient is placed with his face downwards, with a large roll of clothing under his stomach, to promote the evacuation of the contents of this organ by the mouth and nostrils. As soon as this has been effected, the patient is turned on his back, and the roll of clothing is put underneath, opposite the lower extremity of the sternum, so as to render this the most prominent point. Artificial respiration is then instituted, an assistant pulling out the tongue and managing the arms very much as in the method of Silvester, while the surgeon, kneeling beside, or astride, the patient's hips, applies the balls of his thumbs on each side of the pit of the stomach, the fingers lying in the lower intercostal spaces. Using his knees as pivots, he throws all his weight forwards upon his hands, which, with a grasping effort made at the same time, effectually compresses the most yielding and elastic portions of the chest between the hands and the opposing roll of clothing. Gradually increasing the pressure for about two seconds, he suddenly relinquishes his hold with a kind of push, which restores him to the kneeling posture, in which he rests for a similar space, when he proceeds as before, repeating the alternate movements about fifteen times in a minute."

Mr. Gross lays much stress on the use of flagellation in the



attempt to resuscitate asphyxiated persons, which he considers, next to artificial respiration, "the most valuable element in the treatment of this class of affections." The flagellation may be made with the "bare hands, a bundle of thin switches, a lash made of thin pieces of cord, or the fringed end of a towel, used either dry or wet with cold water, the latter mode being particularly serviceable in warm weather."

The method of dealing with blood effused into the pleural cavity, recommended by Mr. Gross, is not in accordance with the practice followed by many distinguished surgeons. We consider that it would be sounder surgery to inculcate the avoidance of opening the pleura, if it can possibly be avoided in hæmatothorax. His observations on this important question are as follows:—

"When the blood proceeds from the lung, a circumstance, however, which cannot always, or, perhaps even generally, be ascertained, the most judicious plan, probably, will be to let it remain, in the hope that it may exert a favourable hemostatic action upon the wounded part; but as soon as all apprehension is over in regard to a recurrence of the bleeding, as it generally will be in five or six days, the effused fluid should be evacuated by operation, either by enlarging the original wound, or, if this is situated unfavourably, by making a free opening through one of the intercostal spaces at the most dependent portion of the chest, or wherever the results of percussion and auscultation may unite in locating the extravasated substance."

Our objection to this interference is, that blood, when effused into the pleura, usually coagulates rapidly, and, unless of very small size indeed, the coagulum could scarcely escape through any but a very free opening. Again, air would enter the pleura through the opening which might lead to pleuritis, hydropneumothorax, and to putrefaction of the blood. Moreover, it has been proved by experience that blood is not so irritating a substance as surgeons at one time supposed. Lastly, when coagulated in the pleura, it may act as a mechanical hemostatic and prevent further bleeding. If, however, air and blood both enter the pleural cavity at the time of the accident, an operation will probably be required.

Mr. Gross very properly advocates early operation for idiopathic pleuritic effusion, so warmly inculcated by Mr. Bowditch, with whose name this department of surgery is so honourably connected. It can scarcely be disputed that the earlier paracentesis is performed the greater the chance of recovery. Plastic lymph so rapidly becomes

organized that the necessity of an early operation becomes apparent, in order that the lung may expand and anticipate such organization. We should observe that we speak of cases in which medical treatment fails to cause early absorption of the effused fluid. When lymph has become organized, it forms part of the body, and sorbefacients cannot then avail against it.

We are of the same opinion as Mr. Gross, that "it is great folly to attempt to lay down any specific rules for the conduct of the practitioner in such an affection as hare-lip." "As to the proper age when the operation should be performed, nothing definite can be said. In the more simple cases," he does "not hesitate to resort to it within a few days after birth, especially if the child is fat, plump, and well nourished; if, on the contrary, he is feeble, puny, anemic, jaundiced, or otherwise sickly," he "postpones it until he is sufficiently strong and healthy to bear the shock and loss of blood, if it requires weeks or even months to accomplish the object."

When there is no contra-indication, we prefer early operation, particularly in the complicated cases, in which, according to Dieffenbach, the mortality is very great, owing probably to the largeness of the chasm, or oral cavity, allowing unwarmed or inspired air to pass too freely to the lungs.

Mr. Gross gives an illustration of an ingenious modification of the ordinary ecraseur, the invention of Dr. S. W. Gross, for the removal of hypertrophied tonsil.

Mr. Gross has lately met with four cases of severe bleeding after excision of the tonsil, in addition to others mentioned by Dr. S. W. Gross. In one of these cases a quart of blood was lost! In two of them the local application of muriated tincture of iron stopped the bleeding. But in two others it was only restrained "by firmly compressing the gland with a stout pair of deeply-serrated forceps."

In the cases of diphtheria we have treated, the debility was so extreme, we could not have ventured to use calomel, even in minute doses, as recommended by Mr. Gross in this affection. Neither could we have ventured to follow his recommendation and use "emetics and purgatives, followed by diaphoretics." We would place more reliance in the treatment of diphtheria, as we have seen it, upon quinine, iron, and brandy internally, and locally, upon one or other of the applications mentioned by Mr. Gross. We have not found mercury to be a safe remedy in the treatment of aplastic inflammations.

Mr. Gross speaks highly of Dr. Sheldon's hernial truss, in which the pad is made of boxwood, and is of a semi-circular shape, "and is connected with an oblong compress, composed of the same material, but much smaller, and so arranged as to bear upon the inguinal canal, while the pad itself supports the abdomen above."

This, we have no doubt, is a very excellent instrument; but in Ireland we are prejudiced in favour of the ingeniously constructed truss of Mr. Francis L'Estrange,<sup>a</sup> the pad of which has bellows-like movements, and acts in such a way that its greatest pressure is effected upon the internal abdominal ring. Many radical cures of hernia have resulted from the use of this truss by Irish surgeons.

Mr. Gross reproduces the following statistical information concerning Wutzer's method and operation for the radical cure of hernia, performed, with slight modifications, by Professor Rothmund, of Munich. Up to 1853, out of 140 cases, "117 were cured, 4 were ameliorated, 6 were not benefited, and 13 relapsed. Of the latter, some were radically cured by being operated on the second time."

Of the amount of reliance to be placed upon these statistics, it is difficult, as Mr. Gross observes, to "form a correct estimate. Rothmund himself states that many of the patients were lost sight of immediately after the operation, while, on the other hand, the cure in a great number of others was ascertained to be perfect at the end of a year and upwards. In this country the operation of Wutzer has so signally failed, that" he is himself "unwilling to put much faith in these assertions."

Dr. Otto Weber, of Bonn, states, in a communication to Mr. Birkett, "that of fourteen persons operated upon by Professor Wutzer, not one was radically cured, but that first, the plug of skin is, by degrees, entirely drawn out again; secondly, that the true herniary apertures, the external and internal rings, are not closed by the operation; and, thirdly, an imperfect cure may be effected by means of a partial closure by adhesion of the internal walls of the neck of the hernial sac, and thickening of the surrounding connective tissue."

From our observation of this operation we believe it to be most inefficient, for the pressure of the wooden plug causes the posterior wall of the inguinal canal to recede towards the abdomen; and being,

<sup>a</sup> Manufactured by Messrs. Fannin & Co., Grafton-street, Dublin.

therefore, unsupported behind, it and the posterior portion of the invaginated tissues are ineffectually maintained in contact.

Mr. John Wood's operations for the radical cure of hernia seem to have been very unsuccessful in the hands of some American surgeons. Thus, Mr. Gross informs us that, "of 22 completed cases from the practice of Dr. Cheever and of the Boston City Hospital, 3 were permanently cured, 3 were much relieved, 2 died, and 14 failed.

Mr. Gross is, himself, of opinion that—

"The most rational radical treatment of hernia is, undoubtedly, the direct, as it may be termed, consisting in cutting down upon the parts, refreshing the edges of the opening of descent, and approximating them with wire sutures, either permanently retained, or until complete consolidation has been effected. The operation, it will be perceived, is founded upon the same principle as that for hare-lip and cleft-palate, and will, if properly executed, be much more likely to answer the purpose than the process of invagination, now so much in vogue, and, for the most part, so worthless. The proceeding is easy of execution, and does not, if the system has been properly prepared by rest, abstinence, and other means, involve any particular danger. It is, of course, not so applicable to ruptures with large openings as to those of an opposite character, as it would be difficult in such an event to effect accurate contact. No interference with the proper hernial sac is necessary. The parts, after the patient gets up, should be for a long time supported with a truss."

Many months before we read Mr. Gross's suggestion to perform this "direct" operation, which he tried in two cases with most satisfactory results, we determined to perform an operation of a somewhat similar nature, rather than any of the "working in the dark" operations, which have so often proved worthless. We formed this resolution from our observation of a case of femoral hernia in which one of these "direct" operations was performed with very good results. The nature of the tumour was at first doubtful, and an exploratory operation was performed. After the hernial sac had been exposed, we suggested that an attempt should be made to effect the radical cure of the hernia, to which the operator acceded.

The contents of the sac having been returned into the abdomen, the sac itself was then gently pushed along the femoral canal to the femoral ring. Some areolar tissue was then partially raised and



made to follow the sac; the inner and outer margins of the saphe-nous opening were next approximated with ligature silk; and, finally, the superficial coverings were secured in position.

With the exception of some slight suppuration, nothing untoward followed the operation; and the hernia, which had hitherto caused a life of misery, by baffling every attempt to retain it, became so manageable that it caused no further annoyance.

The operation of Recamier for the cure of fissure of the anus—namely, forcible dilatation of the anus with the thumbs so as to lead to “temporary paralysis of the parts,” and thus afford the fissure an opportunity of healing—appears to be, judging by a case the author mentions, not altogether free from remote unpleasant consequences. In this case, which was reported by Professor N. S. Davis, of Chicago, the forcible dilatation of the anus was followed by “permanent paralysis.”

The nitric acid treatment of certain forms of piles, a favourite practice of Mr. Houston (Dublin), has not produced very satisfactory results in Mr. Gross's hands:—

“I have occasionally,” he says, “used nitric acid for destroying internal piles, but with results not sufficiently gratifying to induce me to recommend its adoption as a means of radical cure, being satisfied that, while it is more difficult of application and productive of more pain and inflammation than the ligature, it is much less certain in its effects, and more liable to be followed by relapse. Besides, the remedy is not always free from danger, several cases having occurred in which it proved fatal.”

Our own experience of this application leads us to acquiesce with Mr. Gross, that its effects are uncertain. But when people will not submit to either the ligature or actual cautery, it then becomes a useful resource, particularly for the “erectile” pile, as well as for the “milder forms” for which Mr. Gross recommends it.

We cannot bring to our mind ever having seen a fatal case result from the use of the acid.

We venture to dissent from the following suggestion of Mr. Gross for the treatment of wounded non-protruding intestine:—

“It is still,” he writes, “a mooted question as to what should be done when the wounded bowel does not protrude at the opening in the wall of the abdomen. When we reflect upon the fact that in all lesions of this kind the great danger is from fecal effusion, and that such effusion is

almost inevitable even when the opening of the intestine is of very small extent, the duty of the surgeon, I think, plainly, is to enlarge the abdominal orifice, to seek for the wounded tube, and to sew up the cut in the usual manner.”

Special investigations regarding penetrating abdominal wounds have led some surgeons to conclusions altogether different from those that Mr. Gross has arrived at concerning many penetrating wounds of the intestine without protrusion. The evidence afforded by their researches shows that effusion is not an ordinary consequence of these wounds. In fact, the records of surgery prove that effusion of intestinal matters is not a common consequence of penetrating wounds of the abdomen. The impediments to effusion consist in the general contact of the wounded intestine with the surrounding intestines, or with the peritoneum lining the muscles; as well as in the circumstances contingent upon and peculiar to intestinal wounds, such as eversion of the mucous membrane, contraction, and rapid peritoneal adhesion. It is well known that effusion of the intestinal contents is much more likely to follow upon rupture of the bowel produced by either falls or blows upon the belly, where the integuments are not even abraded, than upon the simple penetrating wound of the parietes. Even in large wounds in which, *ceteris paribus*, effusion will take place, this commences before the practice inculcated by the author could be carried out, and, under these circumstances, would it be likely to lead to satisfactory results?

We would place more reliance upon opium, perfect quietness, and diet, than upon the operation here recommended, in penetrating non-protruding intestinal wounds.

The supervention of peritonitis upon a penetrating wound of the abdomen would not be positive evidence of the occurrence of effusion.

In his observations on paracentesis of the abdomen, Mr. Gross, alludes to a case recorded by Mr. Gay (London), in which a good recovery followed penetration of the bowel by the trocar. This case may be used as an argument against the treatment recommended by Mr. Gross, that of enlarging the abdominal orifice and sewing up the wound in the non-protruding bowel.

Mr. Gross delineates an enterotome of his own invention, which, so far as we can judge by the illustration, has many advantages over the enterotome of Dupuytren.

In the description of the endoscopes at present in use, there is a

clerical error in the spelling of Dr. Cruise's name, which appears as "Creuse." In this section there are illustrations of the chief portions of Mr. Wales's (Philadelphia) simple endoscope, which, although not so luxurious an instrument to use as the more expensive ones, is, notwithstanding, an excellent instrument for the endoscopic method of exploring the urethra and bladder.

Mr. Gross, true to his practical professional character, has not omitted to lay down the sound precept, not to completely empty the bladder, in retention, at one *séance*, if the accumulation of urine is very great:—

"When there is great accumulation," he writes, "amounting to several quarts, it will be most safe, as a general rule, not to empty the bladder completely at a single operation, but gradually. The catheter is introduced, and half the fluid is evacuated, to afford the over-stretched fibres an opportunity of contracting and regaining their power. Some hours afterwards the instrument is again used, and then the remainder of the urine is withdrawn. When this precaution is neglected, or unavoidable, the abdomen should be supported by a compress and broad roller. Another important rule is, not to permit the patient, especially if he is old or exhausted, to stand up during the operation, lest, the heart's action failing, he should die from syncope."

We need hardly remind the reader that this practice is likewise advocated by many other practical writers.

As we proceed in this work we find equally sound teaching as to the injury that may result from a too-often-repeated catheterism.

"Although I have known men," Mr. Gross mentions, "to draw off their urine a dozen times a day for years with perfect impunity, yet it may be laid down, as a rule, that, in most cases, when the instrument is obliged to be used for any considerable period, it will gradually set up a slow form of inflammation in the prostate gland and mucous membrane of the bladder, under the effect of which, if proper care be not taken, the patient will ultimately succumb. The frequent use, indeed, of any instrument in the urethra and bladder is a great evil, and should, therefore, be scrupulously avoided whenever it is in the power of the surgeon."

Mr. Gross, in his account of the modes of puncturing the bladder, has overlooked Voillemier's infra-pubic operation between the symphysis and penis.

As an illustration of the great pains Mr. Gross has taken to

perfect this work, we should mention that he describes Mr. Francis L'Estrange's (Dublin) sounding-board for intensifying the noise produced by the sound when it strikes a stone in the bladder. This accessory to the sound has been overlooked by many writers on lithotomy.

Mr. Gross's remarks on the selection of cases for the operation of lithotrity are evidently the result of close observation. Were the points he mentions more frequently kept in view when lithotrity is in question, the mishaps that would most probably result from an injudiciously-selected case might be avoided, and surgical literature would not require to be encumbered with a term, such, for example, as the convenient "surgical kidney," to excuse reckless surgery by those who are ambitious of notoriety.

Lithotrity, Mr. Gross lays down, "is inadmissible in the sacculated bladder, in cystitis, acute and chronic, conjoined with a large calculus, in ulcerated conditions of the bladder, in excessive morbid sensibility of the urethro-vesical mucous membrane, on account of the great susceptibility to systemic disturbance from instrumental contact, in morbid growths, in organic disease of the kidneys, and, finally, in persons of feeble health, the subjects of a hard or large concretion." We regret to find in other portions of Mr. Gross's observations on lithotrity, evidence of great recklessness on the part of some of the United States' surgeons:—"On no account," he cautions us, "should the patient be permitted to walk to his home, as in the event of his being an out-patient. Every man subjected to the operation should be treated either at his own house or in the wards of a hospital. Under opposite circumstances, I have known the worst consequences to ensue, although not in my own practice."

For the lateral operation of lithotomy, Mr. Gross prefers the knife he figures in this work. It is a narrow, long, straight-bladed knife, with a dagger-shaped point. But occasionally he uses a probe-pointed bistoury for enlarging the opening in the prostate and neck of the bladder. "In ordinary cases" he incises "the organ only to a very limited extent, and immediately after" enlarges "the opening with the finger, the pressure of which is generally amply sufficient for the purpose." This agrees with our own observation, and is far more judicious teaching regarding the incisions than is inculcated by some even distinguished lithotomists.

In Dublin, frequently, for both the superficial and deep incisions, a long-handled, short-bladed knife is used, the point of



which is ground off to prevent hitching in the groove of the staff when in course of being pushed to the bladder. A long narrow-bladed, probe-pointed knife, however, is sometimes used for enlarging the deep opening.

We were gratified to find, from Mr. Gross's observations on Mr. B. Wills Richardson's (Dublin) stricture-dilator, that he confirms the opinion of Bryant as to the superior construction of this instrument for safety in use.

Speaking of urethral stricture-dilators, Mr. Gross expresses himself thus:—

“In forcible dilatation, long ago practised by myself and others, but at present generally known as the method of Mr. Barnard Holt, of London, the stricture, from the violence employed, is generally torn, the parts giving way at different portions of their extent. Hence, this process is generally distinguished as the operation of *rupture*, laceration, or divulsion. The instrument with which it is performed is some one of the improvements upon the original dilator of Perrève, of which the best are those of Holt, Voillemier, and Richardson, to which Van Buren, Bumstead, Gouley, and other surgeons, have adapted various contrivances for conducting them safely through narrow coarctations into the bladder. Of these, probably the most perfect is the instrument of Dr. Richardson, of Dublin, sketched at Fig. 589.”

Mr. Gross has had remarkable success from Syme's perineal section, which he reserves for “traumatic strictures, and for cases complicated with great induration and fistules.” He has “performed the operation between twenty-five and thirty times, without, in any instance, any bad effects.”

There is a point in the management of persons suffering from urinary infiltration, mentioned by Mr. Gross, which is more in accordance with good surgery than some writers on organic stricture teach. We allude to the question whether a catheter should be introduced after the necessary incisions for giving exit to the infiltrating fluid. Mr. Gross lays down the rule that catheterism should follow upon the incisions. This we believe to be the safest course to pursue, and it was the practice of Sir Benjamin Brodie, who recommended that the catheter should be thus used, because, according to his vast experience, even after the urethra has given way, there is still a large quantity of urine left in the bladder. This we have ourselves observed, and as it is probable that the urine will be ammoniacal, even before the urethra

breaks, Mr. Gross's confirmatory suggestion appears to us to be the best practice in these cases.

Mr. Gross alludes to a case of congenital malposition of the testicle observed by Zeiss, in which the organ "occupied the left side of the perineum, and offered a serious obstacle to the operation of lithotomy." We mention this case to afford us the opportunity for stating that since we commenced the writing of this notice, we saw an exactly similar malposition in an infant. The testicle, as in Zeiss's patient, was situated in the anterior and left side of the perineum. It could be moved backwards and upwards, but not into the scrotum. The tumour it formed in the perineum was isolated from the scrotum by a deep groove. Mr. James Adams and Mr. Ledwich have met with cases in which a similar malposition of the testis existed.

The following observations of Mr. Gross upon the constitutional symptoms that occasionally either accompany or follow gonorrhœa are of much interest:—

"I am," he says, "inclined to regard the supervention of these so-called secondary affections, not as a result of the direct action of the gonorrhœal poison upon the system, but as a mere coincidence, taking its place in a constitution strongly pre-disposed, by hereditary influence, atmospheric vicissitudes, and the debility occasioned by the treatment of the original disease, to the development of rheumatism in various parts of the body, particularly the joints, muscles, and sclerotic coat of the eye." . . . "In warm climates and intertropical regions," Mr. Gross further says, "nothing is ever heard of gonorrhœal rheumatism, whereas syphilis, in its secondary and tertiary forms, is unusually rife. The fact seems now to be well established that at least some of the forms of so-called gonorrhœal rheumatism are only mild forms of purulent infection."

As gonorrhœal rheumatism usually occurs in fair-haired, strumous persons, we can with difficulty bring ourselves to believe in this mild purulent infection theory.

In the chapter on Ovarian Disease, Mr. Gross alludes to some remarkable cases of ovarian dropsy in illustration of the wonderful recuperative powers of nature in maintaining life, paracentesis having been followed as a palliative.

In one of these cases, which occurred in the practice of Nockler and Son, the woman was tapped 122 times in thirty-one years, and altogether about 2,074 quarts of fluid were removed from the cyst. She was in good general health at the age of sixty-nine, after all these operations. In a still more remarkable case, recorded

by Roloff, the woman was tapped 187 times during seven years, and 3,316 quarts of fluid were removed from the cyst.

With Martineau's and Sir Astley Cooper's cases, and some others to be found in the journals and in works which treat of ovarian dropsy, the reader is, we have no doubt, familiar.

Mr. Gross adduces the following reasons in support of the American origin of ovariectomy:

"Ovariectomy is of American origin, having been first performed in December, 1809, by Dr. Ephraim M'Dowell, of Kentucky. The patient, a married woman, the mother of several children, recovered without any untoward symptoms, surviving the operation thirty-two years. The tumour, partly solid and partly fluid, weighed twenty-two pounds and a half. Until recently it was generally imagined that this operation had been devised and first practised in 1776, by L'Aumonier, of Rouen; but in my Report on Kentucky Surgery, presented to the Kentucky State Medical Society in 1852, I clearly showed that the case of the French surgeon was one simply of abscess of the ovary and the Fallopian tube, occurring in a prostitute consequent upon parturition. For the purpose of giving free vent to the purulent fluid, which had for some time escaped by the vagina, an incision, four inches in length, was made along the lower edge of the external oblique muscle, when, the diseased parts being separated from each, the ovary was removed. The organ, which was encysted, was about the volume of an egg, and of great hardness."

This case, we agree with Mr. Gross, could hardly be called an ovariectomy, in the proper acceptation of the term.

M'Dowell performed the operation about thirteen times. One of his cases affords a useful lesson on the best course to pursue in a similar instance when the adhesions are extensive. In this case, when the tumour had been tapped, extirpation was impossible, owing to its extensive attachments; it re-commenced to grow, and gradually regained its former bulk.

We do not know of any surgical operation for the steps of which it is more difficult to lay down precise rules than that of ovariectomy. We have seen coil upon coil of adherent intestine torn from the sac, and yet a good recovery followed. On the other hand, we have known death to follow the operation in most promising cases, without operative complications of any description. Neither do we know of any disease in which the healthy appearance of the patient assists so little in enabling us to foretell the probable issue of the operation. If we might judge by our own observation,

we should say that the middle-aged, "dried-in" subject, would have a better chance of life than the succulent and younger person.

Through an oversight of the printer, we find in the description of gangrene of the vulva, Mr. Kinder Wood's names joined together thus—Kinderwood.

Clitoridectomy—for performing which Mr. Baker Brown had to run the gauntlet of persecution—has been tried, Mr. Gross tells us, in the treatment of epilepsy depending upon the practice of masturbation:—"For the relief of the latter affection," he writes, "the operation has of late years been performed with very happy results in a considerable number of cases. Professor White, of Buffalo, recently reported three instances of this kind, in two of which the operation seemed to have been perfectly successful." . . . .  
 "Professor White has suggested the substitution of the subcutaneous division of the pudic nerves for the more offensive mutilation of clitoridectomy."

"The clitoris has occasionally been removed on account of erotomania, even when it was not materially enlarged. Such an operation is on a par with the amputation of the penis for the cure of Onanism. Nothing could be more absurd."

For lessening voluptuous sexual feeling in the female, clitoridectomy is more or less successful, and is, by no means, an absurd operation when performed for this object. This will be seen in our notice of the last edition of "Cooper's Surgical Dictionary."

The following case, reproduced from the author's description of gunshot wounds, might, with advantage, be engraven upon the memory of the philo-operation surgeon of the present day. The case occurred in a young private of the 2nd Kentucky Regiment, during the Mexican war:—"Here the muscles of the right hip and of the outer and back part of the thigh, as low down as the popliteal space, were almost entirely torn away by a shell, which at the same time denuded the head of the femur and the femoral artery, the pulsations of which were distinctly perceptible at the inner side of the limb. Notwithstanding this horrible mutilation, rapid recovery took place, and when" Mr. Gross "saw the man, three months after the accident, the parts were nearly cicatrised, without much impairment of function."

Mr. Gross follows the practice of those who advocate the application of two ligatures to a wounded artery, "one above and the



other below the opening. This should be done as speedily as possible after the accident, before there is any considerable inflammation or swelling." This we believe to be sounder practice than the application of only one ligature, and being inculcated by a surgeon of extensive observation in a country where gunshot injuries are of every-day occurrence, should have much weight. We are not, however, disposed to agree with Mr. Gross that—"The operation should be performed even when all bleeding has ceased," unless, as he observes, "the patient is obliged to be transported to any distance," and then under very exceptionable circumstances, one of the most important of which is, the danger of immediate death should the bleeding recur.

We hardly think that it would be justifiable, in the majority of cases, to disturb a deeply-situated vessel, unless the bleeding recurs; for, under proper management, it may not be renewed, and, moreover, we may not be certain what vessel has been wounded. "There is no precept more important," says Guthrie, "than that which directs that no operation should be done on a wounded artery unless it bleed, inasmuch as hæmorrhage, once arrested, may not be renewed, in which case any operation must be unnecessary."

One of the most important arguments in favour of this precept is the difficulty of deciding, in many cases at least, what artery is wounded, and what trunk should be tied. "Errors," as Guthrie repeats, "have been committed on all these points by men of the greatest anatomical and surgical knowledge; the trunk of a sound artery having been tied for that of a wounded one, inflicting thereby on the patient a second and useless wound—more dangerous, perhaps, than the original one it was intended to relieve."

This point is well illustrated in the following case, which caused a great sensation in England some years ago, and which, with his observations thereon, we reproduce from Guthrie's Commentaries:—

"Captain Seton, a short man, fat of his age, was wounded in a duel, in 1845, in the upper part of the right thigh, a little above and in front of the great trochanter, the wound being continued across the thigh, its internal opening being about the middle of the fold of the left or opposite groin. He lost a good deal of blood at the time, the issue of which ceased on his fainting. Ten days after the duel his countenance was blanched, his pulse rather quick and feeble. On examining the wounds, that on the right hip (the opening of entrance) was circular, filled with a dry, depressed slough, and there was a narrow, faint blush of redness round its

margin. In the left groin the opening of exit was marked by a jagged slit, already partly closed by a thin cicatrix. There was extensive mottled purple discoloration (ecchymosis) of the skin in both groins, and over the pubes, scrotum, and upper part of the right thigh. In the right groin was found a large, oval, visibly pulsating tumour, its long diameter extending transversely from about an inch and a half on the inner side of the anterior superior spinous process of the ilium to about opposite the linea alba, and its lower margin projecting slightly over Poupart's ligament into the upper and inner part of the thigh. On handling this tumour, it appeared elastic, but firm, very slightly tender, and not capable of any perceptible diminution in bulk by gradual and continued pressure. The pulsation was distinct at all parts of the swelling, and was equally evident whether the fingers were pressed directly backwards, or whether they were placed at its upper and lower margins, and pressed towards the base of the tumour in a direction transversely to its long axis, the parts being for the time relaxed. The femoral artery was slightly covered by the swelling, and the pulsations of that vessel were with some difficulty distinguished in the upper third of the thigh, below the margin of the tumour. This appeared to depend partly on the natural obesity of the patient, and partly on a considerable degree of general swelling of the thigh. Pressure on the femoral artery or over the abdominal aorta did not arrest the pulsation in the tumour, and in the former situation was attended with severe pain. Under these circumstances it was deemed advisable to apply a ligature on the external iliac artery, and give the patient a chance of the occurrence of coagulation in the tumour, and closure of the wounded vessel, before the free re-establishment of the circulation through the femoral artery. In the present case it was supposed that mortification of the limb was all the less likely to occur from the circumstance that the greater part of the effusion appeared in front of the abdominal parietes, and therefore exercised less pressure on the femoral vein than if further extension into the thigh had taken place. The danger of peritonitis was by this proposal made a new element in the calculation; but it was estimated that the chances of this and of mortification of the limb, taken together, were less unfavourable than the chances of immediate and secondary hæmorrhage attaching to the operation of tying the artery at the spot injured. The operation being completed, the right foot, leg, and thigh, were enveloped in lamb's wool and flannel, and the limb elevated on an inclined plane

of pillows, so as to favour the return of blood as much as possible, and prevent venous congestion. The day on which the operation was performed was passed in considerable pain, the patient being restless, and complaining of a sense of burning in the limb. An anodyne, however, secured him a tolerably good night's rest. The day after, the limb was found altogether diminished in bulk, and its temperature equal to that of the healthy limb; no return of pulsation had taken place in the tumour. The same evening some tenderness and tension of the abdomen came on, though the bowels had been kept in a regular state by occasional small doses of castor-oil. In the morning of the second day, pain in the belly, with increased tension, hurried breathing, short dry cough, and tenderness over the lower part of the abdomen, were observed. Pulse quicker and small. Leeches were applied, and three-grain doses of calomel, with a little Dover's powder, ordered every three hours. The symptoms, however, became rapidly worse; the patient complained of severe pain in the right leg, and a sensation of great heat over the whole body, although the actual temperature was rapidly falling below the natural standard. The right leg, also, became cold sooner than the left. At seven, p.m., he became more easy, and expressed an opinion that he should 'do well,' but in little more than half an hour he expired."

*"Examination after Death.*—Swelling and ecchymosis of the right thigh, particularly at the upper part, and in the right iliac region; also swelling and ecchymosis of the scrotum, chiefly in the right side, with general tumefaction of the abdominal parietes below the umbilicus. A wound, into which the little finger could be passed, was on the upper and outer aspect of the right thigh, about three inches below the crest of the ilium, and about an inch nearer the mesial line than the great trochanter, and on the left side another smaller wound, situated about the external aperture of the left spermatic canal. The first-mentioned wound was open; the lips of the latter were partially adherent. The course of the wound was traced from the outside through a dense layer of fat about two inches in thickness (on an average). It had divided one of the superficial branches of the femoral artery, about half-an-inch below Poupart's ligament, and about an inch from the main body of the femoral artery; this had caused a false aneurism. The sac contained about three ounces of blood. Blood was also effused into the cellular structure of the scrotum, and downwards beneath the sartorius muscle. The wound passed through the cellular tissue,

across the pubes, and emerged about the situation of the left external spermatic ring, without having divided the cord on either side, and was quite superficial to the bladder. No other artery appeared to have been wounded. When the parietes of the abdomen were reflected, a considerable quantity of sero-purulent fluid was found in the abdominal cavity; and on different parts of the large and small intestines, patches of acute inflammation were observed, particularly on the ascending arch of the colon. The peritoneum adjoining the wound of the operation was inflamed, and approaching to gangrene: it had not been injured by the knife during the operation. The intestines were unusually large, and distended with flatus. The other abdominal viscera were healthy, but loaded to an extraordinary degree with fat. The ligature had been properly applied to the iliac artery; the vein was not injured; the surface of the wound and the cellular tissue in the neighbourhood of the artery were sloughy. There was some enlargement of the right limb, but apparently no mortification. The femoral artery was pervious; the course of the wound was through a bed of fat, fourteen inches in length, and three inches in depth, over the pubes, and no muscular substance was injured; the blood found in the aneurismal sac was firmly coagulated, and there was no mark of recent oozing from the injured artery."

"*Remarks.*—If this gentleman had been wounded at the foot of the breach in the wall of Ciudad Rodrigo, in January, he might, to his great dissatisfaction, have been one of the eleven officers whom I saw lying dead, and as naked as they were born, on the face of the breach of Badajos in April. He would have been saved by *one* doctor, or an old woman, and a little cold water in 1812, and did die of *seven* in 1845, after an operation most brilliantly performed, but done in the wrong place, even if any operation had been necessary, which it was not. The case is an *experimentum crucis* of principles.

"The *first error* committed in this case was in calling and believing a wounded artery to be a circumscribed, false, or diffused traumatic aneurism. Nothing can be called an aneurism—by which word a dilated vessel, or a diseased, shut, or closed sac, is understood—which has one or more holes in it, made by a ball, or by anything else, the wound or track of which remains open. It is simply a case of wound in which an artery has been divided or injured, and whilst this track of the ball remains open, no ingenuity of argument can make it otherwise. When the external openings made by the ball have closed, the case may then be called, if there be a collection of blood, whether fluid or coagulated, one



of circumscribed, false, diffused, traumatic aneurism, or anything else that philologists may please to designate it. The dissection report proved this case to be simply a small collection of blood—three ounces and a half, or seven small table-spoonfuls—communicating with two open wounds. Calling this an aneurism, or a shut sac of any kind, was then the *first* and fundamental error, as fatal as erroneous.

“The *second* error consisted in the belief, *contrary to all experience*, that any sac or bag, or collection of blood, by whatever name it may be called, having two openings leading to, or into it, and communicating with the atmosphere, could be augmented to any dangerous extent by the further pouring out of blood from an artery of any size, or from any artery at all, without some of such extravasated blood being discharged, or forced out through one or both of the open external wounds in sufficient quantity to show that the opening in the vessel was not closed.

“The *first two errors*, or defects of principles, gave rise to the *third*—viz., the belief that an operation was necessary where none was required, the dissection having proved that the whole idea of the nature of the injury was a mistake: there was no large artery wounded; the small one, which had been wounded, had ceased to bleed; the quantity of blood extravasated did not exceed seven small table-spoonfuls. The third mistake could not have taken place if the first two errors had not been committed.

“The *fourth* error occurred from its being taken for granted that the femoral artery was wounded and that, ascertaining the fact by opening the small swelling, which contained only three and a-half ounces of blood, would be followed by a fatal hæmorrhage; which supposition arose from this swelling receiving a pulsatory motion from its vicinity to the femoral artery—a mistake which should not have occurred; for it had long before been said (page 16 of my published lectures), ‘The motion or pulsation of the swelling often depends on the impulse given to the whole as a mass by the great artery against which it is lying, and not upon blood circulating through it. When blood is extravasated by the rupture of small vessels in consequence of the passage of a wheel over the limb—especially in the thigh where I have seen a swelling containing fluid blood pulsate in an almost alarming manner, until it gradually diminished as the blood coagulated, when the motion became a mere elevation at each stroke of the heart—the *whizzing sound, or thrill*, attendant on a ruptured artery (of a size to require a ligature being understood) is in these cases wanting, constituting a very distinguishing mark of this accident.

“Surgeons fifty years ago were afraid of hæmorrhage from the femoral artery, but the practice of the Peninsular War dissipated such fears. The reason given for not laying open the wound, and looking at the bleeding artery, in this case is ingenious, but not tenable. The patient is said to have lost a large quantity of blood, and if this were even a fact,

which may, however, be doubted, is there a case on record of a serious wound of the femoral artery, such as this was supposed to have been, in which that vessel has been successfully secured by ligature, without the patient having equally lost so large a quantity of blood as to be supposed to be about to die? *It has always been so* ; the reason, however specious, is not valid, and cannot be admitted.

“The *fifth* error arose from imagining that the considerable loss of blood supposed to have taken place would have rendered the patient incapable of bearing more ; for it is a recorded fact, that those operations high up on the femoral artery, from which patients have recovered, have never been done without great losses of blood having been previously sustained ; and if the patient was so weakened that his heart and arteries could not bear the abstraction from their contents of a few ounces more blood, supposing such loss to be inevitable, how could they have power to drive or force the blood through the limb by the collateral channels in a manner sufficient to support its life, when the main trunk was cut off within the pelvis? *They could not do it—they have rarely done it* under such circumstances ; they could not have done it in this case ; and if the patient had not died within the first forty hours of inflammation of the peritoneum, to which accident he ought not to have been exposed, he would have died of mortification within forty hours more, which had already commenced, as shown by the swelling of the limb and pain in the calf of the leg, which almost invariably attend such mortification.

“The *sixth* error consisted in the belief, that if the femoral artery had been wounded, a ligature on the external iliac would have permanently arrested the bleeding. It would, in all probability, have done no such thing beyond a day or two—perhaps even only for the moment. It is a delusion, persisted in notwithstanding the most clear and positive proofs to the contrary. The patient will die of mortification from the want of blood in the limb, if the circulation be not re-established ; and if this should take place, blood must find its way into the lower end of the wounded artery, and perhaps even into the upper, and renew the hæmorrhage.

“If the femoral artery had been *wounded*, as was supposed in this case, but not completely *divided*, it *must*, and *would*, have continued to bleed through the external wound until the patient died, or a ligature had been placed upon it. It has been said, that in the case as it actually occurred, the little artery which was divided, and which had not bled for some days, could not have been safely tied if it had bled again, because it was only an inch long ; but this is said in defiance of every sort of proof which has been given to the contrary.

“As far back as 1815, I said—‘There was no foundation for the theory which declared that a ligature, when placed on an artery such as the femoral, would fail if in the immediate vicinity of a collateral branch, in

consequence of the flow of blood through this vessel preventing the obstruction and consolidation of the main branch for a distance sufficient to enable it to resist the impulse of the blood behind.' This was said from pure practical facts, free from all kinds of theory; and the preparation before alluded to, in the museum of the College of Surgeons, in which I tied the common iliac artery, will show the mark of a simple thread around it, and a single line of adhesion resisting the whole power of the heart, the canal above the spot not being obliterated.

"The *seventh* error committed in this case was in contravening the great surgical precept, formed on no inconsiderable experience during the early part of the war in the Peninsula, *not to perform an operation on an artery until it bleeds.*"

In the treatment of inversion of the nail of the great toe, Mr. Gross describes a mode of dealing with this painful affection, the principle of which, applied in another way, we have found very effectual in relieving it. "I have repeatedly accomplished the same object," he writes, "simply by the removal of the indurated skin and cellular substance at the margin of the toe, without interfering at all with the affected nail. The incision is directed in such a manner as to leave a sloping surface, which, after the cicatrization is completed, bears pressure well, and is an effectual guarantee against future trouble. Everything else is merely palliative, the patient being at last obliged, perhaps after long suffering, to submit to the knife. The barbarous practice, formerly so fashionable, of removing the entire nail for the relief of this affection, cannot be too strongly condemned, as, aside from its cruelty, it often utterly fails, from the distorted condition of the new nail."

We have for some years been in the habit of treating the so-called in-growing toe-nail on this principle, and have always been successful in preventing relapse. Within the last few months, indeed, we succeeded in curing a young gentleman, both of whose great toe-nails had been removed before he came under our observation. In this case large fungations overlapped the outer side of each new nail, which was small, badly developed, and flat, showing how incorrect it is to refer all the mischief in this affection to incurvation of the nail.

We usually remove the overlapping fungations with either potassa fusa or potassa cum calce, which, we fancy, leaves a more firm cicatrix than the knife.

Mr. Gross is almost totally opposed to the treatment of club-foot as practised by Mr. Barwell, of London, and Dr. Sayre, of New

York—namely, in the majority of cases, without tenotomy, but with mechanical appliances, in which vulcanized india-rubber plays an important part. His reasons for not recommending this treatment are that—"As an exclusive practice, it cannot," he thinks, "be too pointedly condemned. While in some instances it answers the purpose admirably, it is certain that in the great majority of cases tenotomy constitutes a most important preliminary step to a rapid and successful cure." His reasons for this statement are twofold. "In the first place, tenotomy, when carefully performed, is a perfectly harmless operation; and, in the second, the treatment pursued by Barwell, Sayre, and others, requires an amount of skill and attention which few practitioners can command."

Mr. Gross is much in favour of the treatment of varix by *caustic issue*, which, he states, has been *eminently successful* in his hands, being "entirely free from danger and always perfectly successful." He admits, however, that the cure "is usually somewhat tedious, on account of the length of time required to heal the issues" formed with the caustic. He has evidently had great good luck with this treatment, but, through an oversight, he has omitted to mention what ulterior benefit was derived from it. It has been found by other surgeons who have tried this caustic treatment, that, as a radical cure for varix, it does not offer more promising results than the other methods recommended by writers, neither do we believe it to be usually so free from dangerous consequences as it has been in the careful hands of Mr. Gross.

The chapter on Excision of Joints is both valuable and instructive, its value being much increased by the excellent mode in which it is illustrated. Every Irish surgeon who takes pride in the reputation of the Dublin School of Surgery will, we have no doubt, agree with Mr. Gross, that "one of the most able and zealous champions of the operation" of excision of the knee-joint "at present, is Mr. Butcher, of Dublin, who has perhaps done more than any one else to reduce it to rule."

The concluding chapter contains a very concise and useful description of special amputations, and is well suited for those who require accurate directions for their performance, but who have not leisure to search for guiding rules in more elaborate works on operative surgery. If the amputations be performed as laid down in this chapter, success, so far as the operation goes, cannot fail to be obtained.

The so-called Teale's amputation, it appears from this chapter, is



"seldom employed," we presume in America, "in the removal of the forearm." Mr. Gross considers it "chiefly advisable in cases of injury attended with great loss of the soft parts," but he also considers that "the ordinary flap and circular operations are generally preferable."

Mr. Gross prefers Larry's amputation for the shoulder-joint, which, we agree with him, is one of the most easy operations in surgery. "Richerand remarked long ago that it might be performed with the same celerity with which an adroit carver separates the wing of a partridge, and nothing is more true, although" Mr. Gross has "occasionally seen a case in which the surgeon consumed time enough, not only to cut up the whole bird, but also to devour it."

Mr. Gross urges that the gastrocnemius and other flexor muscles should be completely relaxed in the management of the limb after Chopart's amputation at the foot.

From neglect of this precaution he says that "the stump is liable to be retracted, so that the cicatrix, by constantly coming in contact with the ground, is apt to ulcerate and cause severe suffering. If such a contingency should arise, the proper remedy will be the subcutaneous division of the tendo-Achillis, an operation which need never be performed in anticipation of this occurrence." In the last case in which we performed Chopart's operation (a few years ago) the stump has remained without any tendency to elevation of the heel, and we have had no occasion to divide the tendo-Achillis.

The directions given by Mr. Gross for the performance of Syme's amputation at the ankle do not exactly coincide with those given by Syme himself. We think, nevertheless, that, with one exception, as good a stump would be as likely to result from the steps of the operation as laid down by Mr. Gross. This exception, however, is an important one. Mr. Gross directs us to divide the sole of the foot by a "perpendicular incision from the centre of one malleolus to that of the other directly across the sole of the foot." It is to the extent of this incision we take exception, being calculated to lead to division of the posterior tibial artery by the knife, prior to its separation into its plantar branches. The immunity of the posterior tibial artery both Mr. Syme and Mr. Gross consider necessary to prevent, when possible, sloughing of the soft parts from deficient nourishment. In order that this vessel may be avoided, Mr. Syme directs that the incision across the sole of the foot shall extend from just below the centre of the external

malleolus in a straight line (or, in the case of a prominent heel, slightly backwards), to a point at the same level at the opposite side. Now, if we recollect that the inner malleolus does not descend so low as the external malleolus, it becomes evident that this incision is not so extensive as the one recommended by Mr. Gross, which ascends to the centre of the internal malleolus. In the one case the posterior tibial artery may be avoided; in the other it is almost sure to be divided.

We fail to find allusion in this chapter to Seymanowski, the originator of the supra-condyloid amputation of the femur, who several years ago, according to Sédillot, recommended this operation. Seymanowski applied the patella, previously deprived of its articular surface, to the divided end of the femur.

Syme, it may here be mentioned, supplanted, in the latter years of his brilliant career, his revived Hildanus-Hoin operation at the knee by the operation named by some writers "Carden's amputation."

"Unfortunately," says Syme, "not being then aware of Mr. Carden's plan, I formed a covering for the bone by cutting it from the calf of the leg, which proved very inconvenient, and so counterbalanced the benefit anticipated, that this operation soon fell into disuse. . . . But the advantages of this operation (Carden) are not limited to its facilities and satisfactory results in the event of recovery, since its great claim to respect and confidence is the safety that attends its performance."

We think that neither James Young's nor Bauden's names should be overlooked when mention is made of amputation by single skin flap, Young's single flap operation having been a single skin flap amputation, and Bauden's elliptical amputation at the knee having been by single anterior flap, in which the flap was taken from the front of the knee and leg. For further information on this matter the reader is referred to the number of this Journal for November, 1871.

In the description of amputation at the hip-joint, through, probably, an oversight on the part of Mr. Gross, an omission is made regarding one result of the operation, to which we have alluded in our notice of Cooper's Surgical Dictionary; we refer to abscess along the course of the psoas muscle—an unpleasant complication, and one very likely to exert a serious influence on the result of this amputation.

Mr. Gross states that Professor Pancoast, in June, 1860, had

the abdominal aorta effectually compressed by means of a tourniquet, to lessen hæmorrhage during an amputation at the hip. He attributes to Dupuytren the invention of an abdominal tourniquet for the same object, which seems, if we may judge by Mr. Gross's illustration, to be identical with the more recent tourniquet of Skey (London) for compressing arteries in amputations. Before we leave this matter we may state that some injustice has been done to Benjamin Bell by many recent surgical writers in the question of arterial compressing clamps, an instrument identical in construction with Lister's abdominal compressor, having been illustrated by Bell in the sixth volume of his *Surgery*. It was invented for compressing the principal nerves to render parts insensible during amputation. Whether or not the instrument was invented by Mr. James Moore (London), Bell does not decidedly say; at all events, Moore proposed to compress by its means "the principal nerves so completely as to render the parts underneath altogether insensible."

We have now brought our task to a conclusion, and have seldom read a work with the practical value of which we have been more impressed. Every chapter is so concisely put together that the busy practitioner, when in difficulty, can at once find the information he requires. Besides, being illustrated with not fewer than 1,042 excellent wood engravings, its value as a work of reference has been greatly enhanced. Of these engravings, Mr. Gross tells us that "upwards of six hundred are original. The remainder have been copied from the large collection in the possession of Mr. Lea, due credit having been awarded in every instance to the original source where the fact could be satisfactorily ascertained."

Illustrations seem to be considered by surgical writers, who are deficient in ethical fellow-feeling, as common property, which is the probable cause of Mr. Gross's inability to trace the ownership of many of the copied engravings to their original sources, each of them having been, we may safely assume, submitted to a repeatedly unacknowledged appropriation previous to its appearance in this work.

We were gratified at the honourable position given to Irish surgery by Mr. Gross; for, with the exception of Bryant's recently-published important, practical, and aphorismatic *Manual of Surgery*, we cannot bring to our recollection any recent work in which the status of Irish surgery is more fairly upheld.

It is not to be supposed, however, that Mr. Gross has neglected to notice sufficiently the surgery of countries other than Ireland. His work, on the contrary, is cosmopolitan, the surgery of the world being fully represented in it. The work, in fact, is so historically unprejudiced, and so eminently practical, that it is almost a false compliment to say that we believe it to be destined to occupy a foremost place as a work of reference while a system of surgery, like the present system of surgery, is the practice of surgeons.

May we suggest that the next edition should be subdivided into four volumes, this edition being so ponderous as to necessitate its being read, in school-boy fashion, at the desk.

The printing and binding of the work are unexceptionable—indeed it contrasts, in the latter respect, remarkably with English medical and surgical cloth-bound publications, which are generally so wretchedly stitched as to require re-binding before they are any time in use. The stitching, on the other hand, of American calf-bound books is so good that their re-binding is but seldom required.

### CURRENT LITERATURE OF INSANITY.

1. *Lectures on Madness in its Judicial, Legal, and Social Aspects.*  
By EDGAR SHEPPARD, M.D., &c., &c. London: J. & A. Churchill. 1873.
2. *Insanity in its Relations to Crime: a Text and a Commentary.*  
By WM. A. HAMMOND, M.D., &c., &c. New York: D. Appleton & Co. 1873.
3. *American Journal of Insanity*, 1872.

THE literature of insanity is every day assuming a more important place in the annals of the progress of medical science. The study of the various forms of mental alienation is one of the deepest interest, leading, as it does, on the one hand, by mysterious paths, into that dim border land between physiology and psychology, which will, no doubt, long continue to baffle the most adventurous explorers; and, on the other hand, into the full sunshine of everyday life, and the broad field of sociology, in which there are now so many enthusiastic writers, who approach each other from the most opposite starting points. To take high rank as an alienist physician



a man must have a many-sidedness, not merely of genius, but of culture, such as few possess. He must not merely be a broad and deep thinker—a student of the general problems of human nature—he must be a keen observer of the peculiarities of individual character, sympathizing with its idiosyncrasies, and delicately discriminating between its eccentricities and its aberrations. He must be pre-eminently a practical man, ready for emergencies, and able to follow up sagacious decision by prompt action; but he must be practical in a grand and comprehensive fashion. He must be capable of no slight exercise of that “discourse of reason,” which Hamlet makes the peculiar attribute of humanity. He must “look before and after,” regarding the phenomena with which he has immediately to deal as the middle terms in a vast series of causes and effects, the investigation of which taxes the powers of the human mind to the utmost. He meets the lunatic, not merely as an unfortunate fellow-creature, labouring under a terrible disease, but as a unit of society whose acts may be fraught with evil consequences to others, and even as a dangerous criminal; not merely as the last term of a morbid series, the scapegoat of the sins of his ancestors, but as the possible progenitor of a still more morbid progeny.

When we consider the vastness of the subject and the difficulties with which it is surrounded, we think we may fairly congratulate those of our brethren who have made insanity their special study, upon the progress which they have made in various directions during the last half century. Not only has the practical treatment of the insane been steadily improving, but the contributions to the literature of the subject have begun to display a philosophic breadth, in their treatment of the problems which come before them for solution, which does them credit. To say we have as yet attained to anything like an adequate science of insanity, would be much the same as to say that we had attained to an adequate science of human nature; but the last fifty years have undoubtedly produced some valuable contributions to such a science.

Dr. Sheppard, in the “Lectures on Madness,” originally delivered at King’s College, and now before us in the form of a not very long and very readable volume, has presented us with a clever series of sketches of most of the ordinary forms of insanity. He writes in a concise, trenchant, unpedantic style, presenting his very definite opinions in a manner well calculated to arrest the attention and excite the interest of students.

In his first lecture he treats of the general prevalence, predisposing causes, hereditary nature, relative prevalence in the two sexes, and in town and country, and exciting causes of insanity; and concludes by insisting upon the importance of early treatment.

The number of lunatics in England and Wales was, according to the last report of the commissioners of lunacy, close upon 57,000, or  $2\frac{1}{2}$  per 1,000 of the population; and Dr. Sheppard asks the important question, whether insanity is on the increase in these countries. This he is inclined to answer in the affirmative, contrary to the opinions of Drs. Maudsley and Robertson; and he assigns the terrible high-pressure of modern life as a sufficient reason for this increase, if such there really be. As to the predisposing causes, he tells us he has come to the conclusion that "a great many seemingly small and trivial circumstances go towards conditioning a disturbance which ultimately eventuates in disease." He mentions as instances the derision of school-fellows, the thoughtlessness and vanity of near relatives, and even the banter to which an absurd name may expose the victim of foolish sponsors. "The neurotic diathesis," he says, "is evoked and developed by the petty teasings of tailless tyrants, but the tyrants were stimulated to action by the thoughtlessness of loving parents and guardians." He insists, and we heartily agree with him in this, upon the necessity for a careful study of the natures, parental antecedents (where practicable), proclivities, temperaments, habits, talents, and aptitudes of the young, by those who have the care of their education; and praises the Bishop of Exeter and the Master of University College, Oxford, for their noble achievements in this direction. "Such men," he justly expresses it, "raise up a barrier against the *neuroses* by co-ordinating and blending into harmonious working the 'morality of clean blood,' and the morality of the Christian life." He then goes on to speak of what Maudsley calls "the tyranny of bad organization," and describes the insane temperament as being characterized by irregularity of features and development, want of perfect co-ordination of movement, and unusual stupidity or precocity of intellect, with strong sexual appetite. He also confirms the truth of Dryden's dictum, that

"Great wits to madness sure are near allied,  
And thin partitions do their bounds divide."

It probably depends in no small degree upon external circumstances whether an individual shall turn out a madman, or a man

of genius, and this of itself points to the supreme importance of education. Many a man, even, is lost for want of some firm, but sympathetic hand to support him at an important crisis of his life, and with a child the influence for good or evil of those with whom he comes in contact is incalculable.

Insanity Dr. Sheppard believes to be the most hereditary of all diseases, and to be transmitted through the mother, rather than through the father; hence the supreme importance of a man's choosing a thoroughly healthy woman for a wife. As to its relative prevalence in the two sexes, he inclines to the opinion that rather more males are attacked; the apparent excess of female lunatics being due to the fact that they stand the imprisonment of an asylum better than men, who, as a general rule, either die or recover. He states that insanity occurs more frequently between the ages of thirty and forty than in any other decade, and that it is more frequent in summer than in winter, and among rural than among urban populations. This latter fact he explains by the want of sufficient warmth and animal food among the children of rustics. The type of disease is less acute among agriculturists than among artisans.

The exciting causes, physical and moral—the latter being by far the most important—are, of course, very various, and very hard to define accurately, and Dr. Sheppard justly complains that asylum statistics on these points are utterly unreliable. He passes in review a few of the most usual causes—loss of employment, or of a wife or child; the high-strung excitement of competitive examinations; fright; and, worst of all, the “rockets and blue lights” of fanatical preachers. It is really high time to get rid of this “gospel of damnation,” which, being the product of morbid minds, tends to drive one half of the world into melancholy and hypochondria, and the other into hysterical ecstasies. There is plenty of room for a sane and wholesome Christianity, on which we can rely for the production of healthy and happy men and women. Among the physical causes which often, after all, cannot be separated from the moral, Dr. Sheppard enumerates intemperance, masturbation, sunstroke, injuries to head, fevers, and under and over-feeding; but intemperance and masturbation he rightly points out to be, as a general rule, symptomatic of morbid conditions, rather than the starting-points of such conditions—the grand question being not so much what they cause, as what causes them? We may remark that this is a very deep question indeed, involving, as it does, the whole subject of

breeding and education—the study of the conditions under which the procreative act is performed, and the conditions to which the child is exposed during both intra and extra-uterine life. It involves, in fact, nothing less than those grand problems of sexual and social hygiene upon which we are, somewhat timidly, entering at present. Dr. Sheppard draws attention to the strange and suggestive fact, that masturbation is frequently associated in both sexes with precocious piety and the manifestation of religious ecstasy or gloom. We may fairly ask ourselves, in the face of such facts as this, whether the intimate connexion which must exist between pure and wholesome religion and sexual purity, is at all sufficiently acknowledged at the present day.

The statistics of recovery from insanity are by no means satisfactory—Dr. Thurnam, who has studied the subject, informing us that of *ten* persons attacked, *five* recover, and *five* die sooner or later during the progress of the disease; and of the *five* that recover, not more than *two* recover permanently. This leads our author to the subject of early treatment, upon the importance of which he insists, at the same time acknowledging that the difficulties which lie in the way of this are very great, insanity being one of the most insidious of diseases, and one whose existence is not readily acknowledged by the patient or his friends.

In his second lecture Dr. Sheppard treats of classification, rejecting the complicated systems of some nosologists as pedantic and unpractical, and adopting Pinel's division into *mania*, *melancholia*, *dementia*, and *idiocy*, with the addition of *general paralysis*. In this and the following lectures he describes the varieties of insanity, and the methods of treatment which his experience has led him to adopt, graphically and concisely. We have only space for a few gleanings from the valuable matter which the volume contains.

One of the most interesting cases which he narrates is that of a young lady, aged nineteen, and engaged to be married, in whom *mania transitoria* of the ordinary hysterical type, accompanied by great violence and obscenity, occurred after scarlatina. In this case a few hours packing in a wet sheet was followed by sleep, copious and very offensive action of the bowels, and complete restoration. In three months after this the patient was married, and has since borne children, having no relapse even during the puerperal state. It is an interesting question, how far such cases as this are similar to those of true puerperal mania, and whether the



same treatment would be justifiable in puerperal cases. We may remark *en passant* that the aperient action frequently produced by the wet sheet may prove a valuable adjuvant in the treatment of this acute mania of women, in which constipation undoubtedly plays an important part, and that active purgation has been strongly advocated in puerperal mania. This form of mania, Dr. Sheppard remarks, is liable to be mistaken for drunkenness, delirium tremens, or the delirium of fever, or of meningitis; and may end fatally—either rapidly, with convulsions and coma, or by becoming chronic, alternating with melancholia, and finally wearing the patient out. He subsequently describes a case of acute mania, with obscenity, delusions, and mischievous propensities, in a gentleman, who, after several relapses, finally recovered. He remarks that the absence of hereditary taint, and of *fixed* delusions, affords ground for hopefulness in such cases, the indications being to clear out the bowels, to procure sleep, and to keep up the strength. The hypnotics he chiefly relies on are *tincture of digitalis*, in two or three drachm doses at bedtime, when the pulse is quick and bounding, and *chloral* under opposite conditions. Where there is abnormal heat of skin with intense excitement, he resorts to the wet sheet, or the warm bath at bedtime—cold water being poured on the head. The “Turkish bath,” he says, “is not adapted to cases of mania as to those of melancholia”—a vapour bath is better.

In speaking of what is called “moral insanity,” and of impulsive insanity, Dr. Sheppard makes some suggestive remarks. He is disposed to doubt the occurrence of cases of moral insanity *absolutely* without delusion.

“No class of lunatics,” he says, “are so confident in the soundness of their own condition. Here, then, is something very like a delusion *in limine*. . . . The characteristics of this temperament are, for the greater part, vanity, restlessness, capriciousness, impulsive action, with general eccentricity of thought and feeling, and not unfrequently a singularly inharmonious physiognomy. . . . The groove in which they run has a border-line on either side, mapping off the land of genius and the land of insanity, and when they get off this groove (as they so often do), the odds are twenty to one that they topple over to the wrong side of what has been called the ‘thin partition.’ Having no capacity to establish an equilibrium between them and external conditions, their ideas of achieving greatness are anything but the measure of their performances, which merge into the realities of disappointing littleness.”

He *does*, however, believe in cases of *impulsive* insanity, in

which there is impairment of will-power, without apparent delusion. There does, in fact, seem to be a distinct line of demarcation between *moral* and *impulsive* insanity. In the one case the conscience is callous, the equilibrium between the patient and his external conditions being lost, and consequently the moral sense. In the other the will-power is defective, the loss of equilibrium being rather in the economy of the patient's own nature, so that impressions from without become transformed into morbid impulses of terrific violence. In the one case the patient is ready to justify his conduct, however outrageous. In the other the conscience may be even abnormally acute, and the patient may suffer the most excruciating remorse for his impulsive crimes. The two forms may, to some extent, co-exist. It is evident that such cases as these tax the acumen of the medical jurist to the utmost, and that it must be the most difficult thing in the world to decide whether moral or impulsive insanity exists, and, where it does exist, how far the patient can be considered irresponsible for his actions. Dr. Sheppard is disposed to think that some medical witnesses have of late attempted "to advance the theory of irresponsibility, upon insufficient evidence of disease." We shall have occasion to discuss the subject more fully in another place.

Of general paralysis of the insane, that most dreadful of diseases, Dr. Sheppard gives us a graphic picture—the *first stage*, with its alteration of habits, ambition, hurry, tendency to "fastness," and "general untidiness of mind," accompanied by pain in the head, irregularly dilated or "pinhole" pupils, and perhaps slight thickening of speech; the *second stage*, in which all these symptoms are aggravated, and the conduct becomes utterly reckless, the patient declaring himself to be "first-rate," and being full of every kind of magnificent project, while the articulation has become impaired, there is want of co-ordination in the limbs, and the hand-writing becomes sensibly affected—words and letters being frequently dropped; and, finally, the *third and last stage* of this tragedy, in which fatuity, paralysis, grinding of the teeth, and epileptiform convulsions, impairment of reflex functions, formation of bullæ and ulcers, loss of power in the sphincters and sleeplessness, are the forerunners of death. An epileptic seizure may be the first external symptom. General paralysis is most frequent between thirty and forty-five. "Men of the lower class seem to be chiefly obnoxious to it; then we have men of the upper class; thirdly, women of the lower class; whilst, according to Dr. Connolly, the disease is practically

unknown amongst women of the highest class." Sexual excesses, especially those of married men, masturbation, and intemperance, together with the high pressure of modern life, are the chief causes.

In speaking of idiocy and imbecility, and of hereditary criminal tendencies, Dr. Sheppard touches upon a subject too frequently passed over in silence by medical men—namely, the fatal effects of parental vices upon the organization of children:—

"It is impossible," he says, "to study the ancestral bearings of these defective creatures, without arriving at the conclusion that they are, for the more part, the outcome of a defiance of physiological and sanitary and social laws, and that they furnish the strongest evidence of the truth of the Mosaic declaration—that the sins of the fathers are visited upon the generations which succeed them." Of the criminal imbecile he says further:—"Shapen in iniquity and conceived in sin—the blended product, it may be, of intemperance, and syphilis, and epilepsy—he is absolutely powerless to rise."

We proceed to the next book in our list, "Insanity in its Relation to Crime," by Wm. A. Hammond, M.D. Dr. Hammond's book is divided into two parts, entitled respectively "The Text," and "The Commentary;" the former consisting of an epitome of three criminal lunatic cases which occurred in France during the present century—the latter of an interesting dissertation upon some of the subjects suggested by these and similar cases. The *first* is the case of Leger, who was executed in 1824, for the murder of a girl, twelve years old, under circumstances of great atrocity; the murderer having apparently first half strangled his victim, then violated her, and finally eaten a portion of her heart and drank her blood. The prisoner manifested no contrition, and remained in a state of impassibility during his trial, and a *post-mortem* examination revealed indubitable signs of cerebral disease. The *second* is that of Jobard, who stabbed a young lady in the theatre in Lyons, on September 15th, 1851. His reason for this crime was, according to his own statement, that he had been leading a depraved life, and felt that death alone could check him in his downward course, and that suicide would have sent him to his last account with a new crime on his head and no time for repentance; but that by the commission of a capital crime, while insuring his own death he insured a sufficient time for making his peace with God. The *third* case is that of a young man aged nineteen, named Jules, who, acting upon a sudden homicidal impulse, shot his step-mother, who was

sitting at table with his father. He had intended to commit suicide, but the homicidal impulse had caused him to change his mind. He subsequently committed suicide at his victim's grave.

In his Commentary on these cases, Dr. Hammond discusses the whole question of moral responsibility both philosophically, and from a practical aspect. He first points out that *sin* and *crime* are by no means identical, *sin* depending upon the intention, *crime* upon the effect upon society. Where the act is perpetrated without intention (as when a man shoots at a bird and kills a friend), it is simply an accident, not a crime. Anything not illegal, he further states, is, in the eye of the law, not a crime, however injurious it may be *per se*. A law is good or bad according as it is just or unjust to the majority; but if it be just to all, so much the better. The primary object of punishment is the safety of society; but it should, in addition to this, aim at the reformation of the offender—an end too much lost sight of hitherto. In the present state of the law a man is punished for the commission of crimes, even when he is completely ignorant that he is committing a crime; and, this being the case, there seems to be no reason why the insane should not be punished for acts for which they are not morally responsible. The injustice is pretty much the same in either case. Some insane persons, while suffering no apparent impairment of intellect, are such monsters of depravity as to be, to all intents, wild beasts of the most treacherous and dangerous kind. Why should not society rid itself of such pests? The force of example and the force of suggestion is, moreover, immense as regards impulsive crime, and the fear of punishment undoubtedly tends to enable men to repress their criminal propensities. "It is not only for the safety of society, therefore, that insane criminals should be punished, but for the sake of other insane, who are not yet entirely deprived of responsibility."

The knowledge of right and wrong, as Dr. Hammond justly says, although not a test of sanity, is a test of responsibility. The individual who possesses this knowledge possesses what Bain calls "punishability." It is, moreover, doubtful that absolutely "irresistible impulse" ever exists, even in the insane. It is more probable that their impulses are merely of abnormal violence. This being the case, it is surely very dangerous to put them outside the pale of the law of conscience and self-control, by treating them as irresponsible. We kill wild beasts without compunction, when they become dangerous to society, and we should be "almost equally justifiable in killing the insane with irresistible impulses to commit homicide,



if we did not possess places in which we could confine them safely." With regard to degrees of punishment, Dr. Hammond has some very good remarks. "Of course," he says, "the punishments awarded to the insane should be apportioned with regard to the nature of the crime and the character of the insanity, and should thus extend from simple sequestration to fine and imprisonment with labour, and, in some cases, even to death—so long as death is, by law, the punishment for certain kinds of homicide. The only forms of insanity, which, in my opinion, should absolve from responsibility, and therefore from any other punishment except sequestration, are such a degree of idiocy, dementia, or mania, as prevents the individual from understanding the consequences of his act, and the existence of a delusion in regard to a matter of fact which, if true, would justify his act. Persons suffering from either of these forms of mental derangement should, in the interest of the safety of society, be deprived of their liberty. . . . Emotional insanity, and volitional insanity, or irresistible impulse, should generally be allowed as much extenuating force as 'heat of passion.' The exceptional should be cases such as those of Leger and the Alton murderer, in which there was a deliberate purpose to commit murder or other crime." Dr. Hammond concludes by expressing his belief that "temporary insanity" (in the sense that an individual may be perfectly sane immediately before, and immediately after the commission of a particular act) does not exist. "Even in the most striking instances of what is called transitory mania, or morbid impulse, the evidences of pre-existent and subsequent disease of the brain will be found, if they be looked for with skill and diligence and intelligence." We quite agree with this general doctrine, and yet we think it must be admitted that there *are* cases of transitory mania (such cases as those described by Trousseau in connexion with the "*petit mal*," for example) which may very fairly be called "temporary insanity."

Most alienists will heartily concur with Dr. Hammond when he lays down the rule that, "an insane person, deprived of his liberty on account of a murder, should never again be allowed to go at large;" and his suggestion that insane criminals should be confined in penitentiary asylums, and never in ordinary prisons, is also, we think, a valuable one.

The "American Journal of Insanity for 1872," contains some interesting articles from which, did space permit, we might make a good many gleanings.

In the January number there is an account of a case illustrative of the remark we have just made respecting "temporary insanity." It is that of Aratus Pierce, a man of previously blameless life, who shot a man named Bullock, the seducer of his sister, under circumstances which quite precluded the possibility of premeditation. The day before the act he had arranged with the father of his affianced for his daughter's hand, and had telegraphed to his employers to expect him in a couple of days. Bullock had invited him to walk down the street, not he Bullock. He had put on the overcoat in which the pistol was, at his sister's request; and after the deed he exhibited all the symptoms of mental shock. Pierce came of an insane stock, and had inherited the hysterical temperament from his mother. The writer of the article suggests that Pierce was one of those individuals of the nervous diathesis, who stand so perilously between sanity and insanity, that a slight shock, "ecstatic joy, overwhelming sorrow, an embolism from the heart, a spasm of the cephalic artery, or even a failure of the vital impulse," is sufficient to drive them over the boundary line. These are just the cases which make it impossible to decide where sanity passes into insanity. Pierce is one of the Hamlets of real life.

In the April number there is a very full account of the career of Edward H. Ruloff—one of the most extraordinary criminals on record. He was in succession a clerk in a store, a clerk in a lawyer's office, a rural labourer, a teacher in a school, a "botanical physician," the head of a country writing school, and a professional burglar, besides engaging temporarily in many other avocations. Robbery and murder were his principal hobbies, and frequently brought him into difficulties. He was suspected, on very good grounds, of making away with his own wife and child, and was actually sentenced to death for poisoning the wife and child of his brother-in-law, but managed to escape from gaol. Another of his tastes was philology, and he even invented an ingenious but absurd theory as to the formation of language. He was finally executed for a burglary with murder. He combined great intellectual energy, and great manual neatness and dexterity, with an extraordinary bungling and stupidity, with regard to certain circumstances. On one occasion, for instance, he appeared in a suit of clothes made from some goods which he had just stolen. His arrogance and conceit were very remarkable; he appeared to be totally deficient in the moral sense; and on receiving his sentence burst into a torrent of the most fearful blasphemy and obscenity. The desire to be of some consequence

(which the writer seems to think a special characteristic of the criminal class), was very strong in Rulloff, and appears to have taken the place of the moral sense. A commission in lunacy was appointed to report on his sanity, when under sentence, and reported him perfectly sane. The only *post-mortem* appearance noticed in the article, is that, as is said to be the rule with criminals, his skull was remarkably thick.

The October number is chiefly taken up with the proceedings of the "Association of Medical Superintendents." There is not much of very great interest in these long-winded discussions. The principal subjects discussed are—transitory and impulsive insanity, and the dangers resulting from the latitude which medical men allow themselves in speaking of these affections; the importance of moral causes in the production of insanity; and the great evils resulting from the disgraceful overcrowding of the State Asylums in America. Two points specially force themselves upon our attention in reading these records of much talking, and these are, the immense difficulties which lie in the way of drawing any distinct line between sanity and insanity; and the great importance of regarding lunatics as responsible rather than irresponsible beings. We have, no doubt, to deal with a physical as well as a mental disorder, in dealing with insanity, but we should never lose sight of the immense importance of the moral treatment of the patient.

*A Treatise on Gout, Rheumatism, and Rheumatic Gout.* By AUSTIN MELDON, Licentiate of the King and Queen's College of Physicians and of the Royal College of Surgeons, Ireland; Surgeon to Jervis-street Hospital, Dublin, &c., &c. London: Longmans, Green, Reader, & Dyer. Fcap 8vo. Pp. 146. 1873.

WE have somehow overlooked Dr. Meldon's work on Gout and Rheumatism, which should have received an earlier notice. As will be seen by the title, it consists of three divisions. Of these, the first is by far the most important, and occupies the greater part of the volume. The author appears to have had a much more extensive experience of gout than falls to the lot of most physicians in this country, where the disease is rarely seen among hospital patients; and he describes, very tersely and graphically, its various forms. He thus states his view of the pathology of gout:—

"The condition of the liquor sanguinis is of the first importance in the production of this formidable malady. The disease cannot be developed

unless the blood contain a considerable quantity of uric acid and soda in some form. Thus far the uric acid theory is correct. But, in my opinion, it is an error to suppose that these two exist in combination before an attack of gout; the moment they combine, the disease is produced. In those who are predisposed to the affection, the union of soda and uric acid is prevented by the influence of the nervous system. When, from any cause, the nerve force is suddenly lessened, the circulation becomes languid, passive congestion occurs, and the uric acid seizing on the soda, forms urate of soda. This is deposited in the tissues, generally in those most removed from the centre of the circulation, inflammation results, and an attack of acute gout is established. The irritation produced by the disease excites the nervous system to unusual energy; this invigorates the heart to increased action and a greater or less amount of fever is set up. The rapid circulation stimulates the almost congested organs, and the kidneys, liver, and skin aid one another in eliminating the uric acid. Thus the fit passes off, leaving the nervous system in a more energetic condition than before, the mental faculties clearer, and the patient finds himself in a far better state of health than previous to his illness."

Whether or not we agree with Dr. Meldon's theory of gout, no one who has had the care of persons who suffered from that disease can doubt the correctness of his assertion, that influences which depress the nervous system have a great effect in determining the occurrence of an attack; and we quite agree with his recommendation that in the treatment of the gouty we should aim both at purifying the blood and giving tone to the nervous system; for the details we must refer to the work itself.

Rheumatism the author believes to be so far analogous to gout that the occurrence of an attack is determined by a similar depressed condition of the nervous system, "allowing the crystallization of lactic acid, or some other chemical change in its composition, which converts that which was previously a harmless constituent of the blood into an agent of irritation to almost every part of the body." In the treatment of acute rheumatism the author advocates a union of the alkaline treatment with small doses of colchicum and opium. As soon as the acute symptoms have begun to subside, he prescribes quinine, and throughout clothes the patient in flannel, and keeps him on a restricted diet, with citric acid lemonade. Under such management, he says, "the disease is generally quite gone in from six to ten days"—a result which those who have seen much of rheumatic fever must look upon as sufficiently brilliant.



## PART III.

### MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

#### TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

*Wednesday, February 11th, 1874.*

SAMUEL GORDON, M.B., in the Chair.

DR. T. W. GRIMSHAW exhibited and described Casella's spirometer.

*A Case of Cerebro-spinal Meningitis, in which Hypodermic Injections of Morphia and Atropia were freely used.* By JAMES LITTLE, M.D., Edin.; Professor of the Practice of Medicine, Royal College of Surgeons.

ON December 20, Isabella S., aged twelve years, was sent into the Adelaide Hospital by Dr. Walter Smith, under whose notice she had come among the out-patients.

On December 16 she had been attacked with severe frontal headache, and so overpowered by a feeling of illness that she could not sit up.

On December 19 she had vomited repeatedly. When I saw her on the afternoon of the 20th she was lying as if in a heavy sleep; she could easily be roused, but at once relapsed into the sleepy condition; the face was flushed; she said she had severe headache, and she manifested marked intolerance of light. An ophthalmoscopic examination, made by Mr. Swanzy, showed hyperæmia of the retinal vessels. She had vomited several times since admission. There was slight subsultus; the tongue was covered with a creamy fur; the bowels had not acted for twenty-four hours; pulse 130; temperature 104·8. Her hair was cut close, a leech applied to each temple, and four grains of calomel given.

The headache was distinctly relieved by these measures. She passed a quiet night, though she did not sleep much, and wandered somewhat. She was put on milk diet, with beef-tea, and ten grains of the bromide of potassium given every second hour.

On December 22, 23, and 24, she was drowsy, but easily roused. She no longer complained of headache, but the face was flushed; the carotids throbbed violently; she vomited occasionally; had slight subsultus, and was very thirsty.

On the 25th marked retraction of the neck was observed. She lay with her legs drawn up, and complained bitterly of any attempt to extend them—or, indeed, of any attempt to alter her position. She cried and moaned incessantly; during the day she seemed rational, but during the night she raved.

On December 26 her condition was unchanged.

On December 27, though she took nourishment freely, she seemed sinking; the heart's action was extremely feeble, and the extremities cold; hyperæsthesia of the surface and retraction of the neck were highly marked; and although the prominent points of her body had been carefully padded with cotton wool, large sloughs were to be seen forming over both trochanters and over the sacrum, and dark patches had appeared on the inner aspects of the knees, where they came into contact, as she lay with her legs drawn up in bed. I now directed the bromide of potassium to be discontinued. I prescribed two grains of quinine every fourth hour, and the administration of a hypodermic injection of morphia and atropia as often as was necessary to keep her easy. The nurse was, at the same time, instructed to feed her carefully, just before each injection, and again as soon as its effect had somewhat passed off; and, in addition to strong beef-tea, eggs, and milk, she had a daily allowance of four ounces of port wine, and two ounces of whiskey. The hypodermic injections were continued for eight days; at first gr.  $\frac{1}{6}$  of acetate of morphia, and gr.  $\frac{1}{120}$  of sulphate of atropia were used, but as this proved insufficient to relieve her, larger quantities were used; three times half a grain of morphia was injected, but on all other occasions a quarter of a grain was found effectual. The injections were generally made three times in twenty-four hours.

The record of her pulse and temperature during this period was as follows:—

	Morning Temp.	Pulse.		Evening Temp.	Pulse.
Dec. 24	—	—	-	105°	124
„ 25	104°	126	-	—	—
„ 26	102°·8	128	-	103°	126
„ 27	102°·8	—	-	102°·6	—
„ 28	103°	124	-	103°·8	128
„ 29	103°·6	130	-	104°·2	133
„ 30	102°·4	124	-	102°·6	128
„ 31	101°	110	-	101°·1	110
Jan. 1	101°	105	-	101°·1	115
„ 2	101°·5	100	-	102°·1	114
„ 3	100°	120	-	99°·8	116
„ 4	99°	120	-	—	—

On January 4 there was a decided lessening in the severity of all the

symptoms, but the morphia was used at night until January 8, when it was discontinued, and from this date treatment was mainly directed to the healing of the bed-sores, which detained her in hospital till the beginning of the present month.

There is no doubt on my mind that this was a case of cerebro-spinal meningitis of sub-acute type, and I think my colleagues, Dr. Head and Dr. Smith, who saw the girl, are of the same opinion.

I saw one of the first cases of cerebro-spinal meningitis which occurred in Dublin during its epidemic visitation, and on the first night of the gentleman's illness, when restlessness, pain in the head, and hyperæsthesia were marked, I gave him a full dose of Battley's sedative liquor by the rectum, with the effect of at least greatly lessening his sufferings for a time. Subsequently, in compliance with the views generally held as to the impropriety of giving opium in cases of the disease, I abandoned its use; but having long ago satisfied myself that we may, with safety and great comfort to the patient, resort to the hypodermic injection of morphia for the relief of headache due to meningeal inflammation, as in cases of cerebral tumour, I have used it in the few cases of cerebro-spinal meningitis (I think five) which have come under my care since the subsidence of the epidemic, and which were marked by hyperæsthesia, spasm, excitement, delirium, and acute suffering. In those cases in which collapse or coma was the prominent phenomenon, I have not had recourse to it.

Though, I think, but very seldom and with hesitation prescribed by Dublin physicians during the last epidemic, opium, as stated very strongly by Stillé, has been by many observers esteemed of signal use in cerebro-spinal meningitis; and, strange though it may appear, has been used as a remedy against the coma, which is the prominent symptom in many cases; and I venture to suggest its more frequent employment, at least in cases such as the one I have narrated.

The CHAIRMAN said that it was admitted by all physicians to be a matter of very great importance, and a proper line of treatment in many of these formidable diseases, to treat the symptoms themselves as they arose. That plan often gave time for further suitable treatment to be directed to the disease itself, and, what was perhaps of as much importance, gave time for the law of periodicity to assist in subduing the disease. The efficacy of the plan of treatment detailed by Dr. Little for relieving the pain was most satisfactory, because there was no doubt that many of these cases had died from the amount of pain and suffering they had undergone.

DR. MACSWINEY should like to know how much morphia was given in the twenty-four hours. One quarter of a grain in each injection would

amount to a formidable dose of morphia for a child twelve years old. He should like to ask Dr. Little, in the second place, whether, in his opinion, atropine acted as a neutralizer of opium, or strengthened its action, as some believe?

DR. GRIMSHAW said, although he had seldom used either morphia or atropine in the way Dr. Little had mentioned, he had used other preparations which involved substantially the same thing in cerebro-spinal meningitis. He had treated about forty cases during the last epidemic, and treated them in various ways. Generally speaking, the plan he pursued was to give them belladonna in very considerable doses—the object for which he gave it being to allay the great hyperæsthesia, rather than for any other purpose—and it generally succeeded in producing the desired effect, giving the patient great comfort, enabling him to sleep, and diminishing considerably the cutaneous sensibility. He thought it was not so good as giving atropia itself, for he rather disapproved of giving complex drugs when they could give the active principle. He had given opium in several cases, but in two cases only had he used subcutaneous injection of morphia, and it made the patients comfortable, and they both recovered. They were not, however, severe cases, being unaccompanied by black spots, or any of the more malignant symptoms of cerebro-spinal meningitis. In the last case which he treated he gave bromide of potassium, but the case proved fatal. These cases would bear an immense amount of opium without any serious symptoms. In one case he gave twelve grains of opium within twenty-four hours, and the patient was very much benefited by it. That was a peculiar and a very serious case. The patient got no hypodermic injection, but opium with iodide of potassium. The disease set in very suddenly—the patient had black spots and partial hemiplegia. He recovered, except as regards the paralysis of the right arm, which, however, was better when he last saw him, and he had since heard that the man, who was now in England, was perfectly well. He believed the opinion of the American physicians was correct, and that opium would be generally beneficial in these cases. He had used belladonna in at least twenty cases, and it always appeared to him to allay the hyperæsthesia, and in these cases the patients appeared to be worried to death by pain and annoyance. The mere prevention of pain was a great advantage to them, and he believed it absolutely saved their lives in some instances. The lighter cases he did not count upon much, for he thought the disease possessed very much of the nature of a specific fever, and where the cases progressed regularly, grew up gradually to their acme, and then subsided, they would generally get well of themselves, just as cases of typhus or scarlatina would. He, therefore, spoke only of the severe cases that recovered, and he had a number of very bad cases, with black spots, &c.



DR. WALTER SMITH said the case described by Dr. Little was gratifying, because of the recovery of the child from a grave assemblage of symptoms, but it was more valuable as an indication of the modification of the action of drugs in certain cases. Very large doses of morphia were given in this case to a young child. They all knew that there were two very distinct lines of action in all these opium alkaloids—the stimulant and the narcotic; they knew that in children the physiological influence of opium was its narcotic action—predominating, to the exclusion, he might say, of its stimulant action; and it would seem that in some cases of disease, possibly in some cerebral cases, the narcotic influence of opium appeared almost to remain latent, whereas the stimulant action came out in full force. It might be worthy of chemical observation to ascertain if there were cases, and if so what class of cases they were, in which the one tendency of the drug appears to be absent, or greatly diminished, whereas the other stood out in full relief. Viewed in that direction, the case described by Dr. Little opened up a field for future inquiry.

DR. DARBY said, as he believed he was the first person in Great Britain who had brought cases of cerebro-spinal meningitis under the notice of the profession in this country, he could not allow the present discussion to close without making a few observations upon Dr. Little's very interesting case. He had had no experience of the action of opium in the treatment of this disease; but he had been very much struck by the changes presented in Dr. Little's case, and by his treatment, as also with the difference of symptoms he had observed since the disease had first come under his notice. The difference between the acute form of cerebro-spinal meningitis, when it first appeared in his hospital in the year 1846, and such a case as the present, was very remarkable. In that year the symptoms were much more acute than any he had since observed, and he did not believe that any man in his senses would have given narcotics in those cases, where nothing but bleeding and calomel saved life. He had, since that time, seen the disease with all sorts of modifications—sub-acute, very mild, and chronic. Many cases of that remarkable epidemic which occurred in the years 1867–68, presented the complication of cerebro-spinal disease, together with blood-spots; but as he had seen very many of those cases where the eruption of blood-spots was copious, quite free from all symptoms indicative of cerebro-spinal excitement, he was disposed to consider that epidemic had been essentially one of what he believed Dr. Stokes had named purpuric fever, and he thought the case Dr. Little had brought forward ought to be regarded as a case essentially of fever, with cerebro-spinal complications. Regarding it as such, he considered Dr. Little's treatment was most judicious, sustaining the patient through the fever, until the organic

complications had subsided. Dr. Mayne had recently asked him (Dr. Darby) to see a case—a child of six years old, who had got tetanus. Dr. Mayne diagnosed the case to be one of cerebro-spinal meningitis, and treated the child by calomel and blisters to the spine. The child had trismus, and was in a state of opisthotonos, with a pulse of 120, and altogether a very unpromising case when he (Dr. Darby) saw it with Dr. Mayne. The child recovered under this treatment.

DR. LITTLE, in reply to Dr. MacSwiney, said the injection was repeated every six or eight hours. The amount given was always enough to quiet the patient. As to the union of morphia and atropia, the subject was too wide to enter upon there. The solution which he generally used was one, five minims of which contained gr.  $\frac{1}{6}$  of morphia, and gr.  $\frac{1}{120}$  of atropia. He admitted that this was not an acute but a sub-acute case, but it was a case that was going to die, as far as he was able to judge. As to the opinion of the American physicians with regard to opium (referred to by Dr. Grimshaw), Dr. Stillé speaks of opium having been chiefly given for its stimulant effect, and says that it was given even in comatose cases. He was obliged to his friend, Dr. Darby, for having criticised his diagnosis. He thought, however, the case was not one of the ordinary essential fevers. The range of temperature showed that it was not one of enteric fever, as did also the absence of eruption and the character of the stools, whilst the ophthalmoscopic examination made by Mr. Swanzy proved the existence of meningeal mischief.

### *A Case of Hysteria.* By S. M. MACSWINEY, M.D.

THE case which I purpose detailing to the Society this evening presented such a number of the graver complications of the disease of which it is a typical example, that I have considered it might be of some interest to relate it.

A. D., a young woman, aged twenty-four years, with black hair and brown eyes, of a somewhat dusky, sallow complexion, and presenting a lax condition of tissue generally, was brought by the police to Jervis-street Hospital at 5 a.m., December 17, 1873. The police stated that they had arrested her some hours previously for disorderly conduct in the streets, and that she had, after a time, become insensible in the lock-up cell. She certainly appeared to be quite insensible when admitted into hospital, and remained so for about half an hour. There was no smell of alcohol about her, and she did not in the least convey the idea of being under the influence of drink. After the lapse of the above-named time she suddenly started up, uttered a low sound, ground her teeth audibly, and was seized with a “fit,” in which she was violently, indeed terribly, convulsed. This “fit” was repeated at short intervals, during the ensuing

six hours, and her state was so alarming as to impress those around her with the fear that she would die in one of the attacks. Her condition during a "fit" was as follows:—She usually, but not always, uttered a sort of growling or muttered sound first; then her eyes were turned obliquely up, and buried in the orbits, so that the irides were invisible when the lids were separated; the eyelids were partially closed and quivering, the teeth were audibly ground together, the arms outstretched and rigid, the hands clenched tightly, and the trunk and extremities fearfully convulsed. The character of these strange convulsions was singularly marked and distinctive. She rested for an instant on her back, her trunk was arched forwards, so that her head and heels alone were in contact with the bed; then she suddenly became agitated in an indescribably horrid way over her whole frame; she plunged backwards repeatedly with great force, then bounded—now to one side, then to the other side of the bed, in imminent danger of killing herself by striking the bedstead with her head, and requiring the efforts of four persons to restrain her. The spasms were very remarkable; I often observed them attentively, but I know not how to describe them except by saying that they were varied, irregular, inconceivable, and indescribable. During a paroxysm she *seemed* to be quite insensible, but many circumstances—notably the tremulous action of her eyelids, increased when the hand was pushed towards her face and shaken at it—served to produce a conviction in the mind of the bystander that she was not, by any means, wholly insensible to what was passing around her. We felt assured that she was partially conscious, not in the least malingering, but having a consciousness and will so morbidly affected as to be wholly unable to control her abnormal contortions and movements. Whilst in a "fit," which was not greater on one side of her body than on the other, but engaged the whole frame at the same time in an equal degree of violence, she did not *cry out*, or bite her tongue, or froth at the mouth. When the fits had lasted, with short intervals of repose, for a period of about six hours, they ceased; she appeared to be completely exhausted, and sank into what seemed to be a quiet, sleep-like state of rest. After some hours lapse she roused up, and was now evidently conscious, but could not be got to return any vocal answer to questions. She evinced the utmost dislike to speak, the only reply she would make when addressed, being by nodding or shaking her head, or by some other sign or gesture. Thus, upon being asked if she was suffering pain, she nodded, and then, when asked the site of the pain, she placed her hand under the left breast, but would not utter a word.

I may here state, once for all, that during the six weeks the patient remained under observation in hospital, she had very many returns of these frightful spasms, that they were always alike in character, such as I have described them, and that we soon discovered that she was far

more likely to get them—and, in fact, often did get them—when any of the medical attendants entered the room, than when they were absent therefrom. At the same time it must also be said that they came on occasionally in the stillness of night, when she had previously been, to all appearance, pretty well and seemingly asleep, and when all was quiet about her. When thus unexpectedly attacked, the utmost consternation would be excited amongst the other inmates of the ward, and in this way there existed, almost constantly, a state of excitement, more or less according to circumstances, in the room. These convulsions were the most striking of her morbid manifestations, but it will be at once surmised that they were not the only ones she presented. In fact, she exhibited many other varied and serious phases of disease, and amongst them the following may be mentioned as the principal additional symptoms presented by her whilst she was under observation:—

A. *Motor Lesions*.—1. The most serious and remarkable of the derangements of motor power were the convulsions already detailed.

2. She presented a mild form of a paralytic condition of the lower extremities. When allowed to get out of bed she walked with great apparent difficulty, dragging her feet along the floor, and by her manner implying a loss of motor power in her limbs. When caused to stand up, too, she staggered from side to side, and seemed in danger of falling unless sustained by an assistant.

3. *Retention of Urine*.—Great disturbance of the innervation of the bladder was observed, and she frequently lost the power of emptying that viscus for days. I have found the distended bladder forming an enormous tumour in her abdomen, and have taken away over sixty ounces of water at a time. She made no complaint of inconvenience from the retention, and did not even appear to have been aware of its existence.

4. *Constipation*.—The bowels were obstinately confined all through her stay in hospital, and it was necessary to apply remedies at regular intervals to overcome the costiveness.

5. *Tympanites*.—This morbid state was very well marked. The abdomen, for about a week, was greatly distended with air, forming a huge tumour, resonant to percussion, larger than that of the uterus at the full period of gestation. Remedies were quite ineffectual to remove the accumulated flatulence, which, however, caused no suffering. It afterwards disappeared spontaneously.

B. *Lesions of Sensation*.—Derangement of sensibility was manifested in a variety of ways. Amongst these disturbances of sensory function I will mention but two.

1. *Pain*.—She constantly complained of pain; its site was either under her breast, in her back, in her feet, in the epigastric region, or in her head. When pressure was made over the dorsal vertebræ, in the spine, she cried out in seeming agony. Upon the many occasions when I



questioned her respecting this symptom, she never once admitted that she was without pain.

2. *Hyperæsthesia*.—There was well marked increase of sensibility in the lower extremities. The slightest touch to either of the feet or legs was followed by instant retraction of the limb, and a complaint, forcibly expressed on her countenance, of great suffering.

C. *Psychical Lesions*.—1. This patient, during almost all the time she was in hospital, was plunged in the most profound melancholy. She ever kept her eyes cast down, unwilling to speak, and disliking to be spoken to; refusing food or drink except on rare occasions, never smiling, and presenting at all times an aspect of the utmost sadness. Usually she was neither capricious, peevish, nor exacting in manner, she did not seem even to desire sympathy, but she always spoke most despondingly of herself, as of one who was unworthy of being either succoured or pitied.

2. *Hypochondriasis*, which, I assume, is a symptom of a diseased mind, existed in a very developed state in this patient, and constituted a troublesome item in her daily complaints. Her imaginary disorders were countless, and need not be enumerated here.

3. On one occasion I found her, when I entered the ward, in a very alarming state. She lay, seemingly unconscious, on her back; her mouth was open; her tongue, dry, rough, and brown, was slightly protruded; there were sordes on her teeth and lips, and she was respiring fifteen times in the minute, with a slight stertorous sound. I was told she had been found in this state in the morning, and had manifested no further sign of life since than that I have described. Her temperature was lowered, and her skin was cold to the touch; her pulse was weak and slow. I certainly thought it not improbable that she was dying. Active treatment, of a stimulating character, was had recourse to, and as she either could not or would not swallow a single drop of any liquid, nutrient and alcoholic enemata were given, and vesication of the spine was induced. She remained in this coma-like and serious state for about fifty hours, when she gradually seemed to revive, and finally emerged from it into her usual condition.

D. *Symptoms referable to functional derangements of the stomach and digestive organs*.—1. She had, for the greater part of the time, complete loss of appetite. At times this inappetence approached to the condition of "hysterical anorexia," so graphically described by Laségue (*Med. Times and Gaz.*, Sep. 6, 1873), and by Sir Wm. Gull (*Med. Times and Gaz.*, Nov. 8, 1873).

2. But at other times she would eat eagerly, almost voraciously, and what is very singular, her appetite was at its very best at that particular time when she was complaining most of pain in her stomach.

3. Nausea appeared to afflict her very much; she used to wish to vomit, and often brought up a quantity of glairy mucus.

4. Under this head must be included a symptom which is one of the very gravest, and at the same time most unusual of the occurrences in the disease, and which is to be accounted for—I should suppose—by the occurrence of a great determination of blood to the stomach, caused by derangement of innervation. The symptom I allude to is *hæmatemesis*, which prevailed to a considerable extent for eight or nine days. Each day during this time, as I entered the ward, I found beside her bed a vessel containing from two to eight ounces of a bloody liquid of somewhat peculiar appearance. It was very dark in colour, thin, homogeneous, glairy, without air-bubbles, free from smell, not containing clots. I never myself saw her bring this fluid up, but I was informed by the patient and those about her whom I instructed to observe her, that she ejected it after some straining efforts, as it would appear, to vomit. This occurred at the time when she had the most severe gastrodynia, and when also her appetite was at its best. I may here, appropriately perhaps, digress for a moment to recall attention to the fact that it often becomes necessary to distinguish between pain in the stomach, due to ulcer there, and hysterical gastrodynia. Vomiting of blood is a most important symptom in ulcer of the stomach; and its existence much facilitates the diagnosis. But in this case of undoubted hysterical pain, we have “*hæmatemesis*” as one of the grave symptoms, a fact well worthy of being borne in mind—showing, as it does, the possible occurrence of an unusual symptom in the disease.

E. *Uterine symptoms*.—1. Morbid states of the uterus often, perhaps usually, co-exist with hysteria, and in such instances are regarded generally as exciting causes of the disease. Hysteria, however, is sometimes met with in men, and it may exist in the female, although the vagina, uterus, and probably the ovaries (Dr. J. Castiaux, *Bulletin Medical du Nord*, No. 4, 1873), be absent, and consequently the disturbed innervation manifested in such cases cannot be, in any way, related to a connexion between the nervous trunks and these organs. Still, usually in hysteria, there is some deviation from the healthy action of the uterus. In this case all that could be made out, after a careful examination, was the existence of a considerable leucorrhœa, and a somewhat abnormal redness of the os and cervix.

2. She had been, she said, pregnant twice, and had miscarried at about the fourth month in each instance. She never had a child at the full term.

The pulse was often very weak, but never went beyond the normal number of beats, not even after a “fit,” whilst the temperature never exceeded, but occasionally fell below, the physiological standard. The urine was slightly acid, deep colour, sp. gr. 1030, and did not contain albumen.

At first it was not quite certain in what category the illness suffered

by this patient should be placed, but the symptoms manifested, carefully observed, soon supplied the means whereby the differential diagnosis was made, and the conclusion arrived at that it was neither epilepsy, trismus, tetanus, nor eclampsia, but a well marked case of confirmed hysteria, that we had to deal with.

This poor girl's story was a sad one; she had evidently been rather carefully brought up, but she early fell a victim to seduction, and after an abortive attempt at reformation for a brief period, finally abandoned herself, as we were informed, to a loose life in Dublin. She did not appear to have, at any time, yielded to habits of intemperance. She seemed to be constantly afflicted with remorse and vain regrets, and frequently said she was not fit to be amongst the properly conducted people around her.

Having related the particulars of this case I have little more to add. I make no attempt to explain the true nature of a morbid state which, in its Protean forms, has baffled the skill of so many investigators, and whose cause may be said to be still involved in the deepest obscurity. In a confirmed case there will usually be found derangements of the *motor*, *sensory*, and *psychical* functions, together with great disturbance of the digestive system. The record of the case which I have detailed shows that each of these lesions was represented by some symptoms, suffered from by the patient, and were I to extend this paper further, in the direction of an inquiry as to the etiology of the disease, I should be able to do little more than enlarge upon the groups of morbid phenomena I have depicted as presented, and whose combination constitutes confirmed hysteria. I will only then further say that it seems to be generally held that in a great many cases, probably in most, of hysteria, there is disorder in the sexual organs, and that the nerves most immediately affected transmit the disease to other nerves and to central organs. It was in this way that Todd used to explain, in his lectures, the submammary pain as being sent by nervous force from the ovary to the spinal cord, and thence reflected, by a similar agency, to the side.

Leucorrhœa was the only morbid uterine state present in this patient, and quite possibly this lesion of the womb is competent to act as excitant of hysteria in a pre-disposed subject. But I am disposed to ascribe this patient's hysteria to *psychical* rather than to *physical* influences. The miserable state of her mind, brooding over her wasted life, developed the morbid state of her nervous system, which culminated in hysteria.

DR. MARCUS EUSTACE said he had listened with much interest to Dr. MacSwiney's paper, because he had had cases exactly similar under his care lately. He had one which, if he detailed carefully the symptoms, would be found analogous with that just described, but some of the symptoms were more serious. He should like to know how Dr.

MacSwiney's case terminated, whether by immediate or prolonged recovery? He had known where recovery had taken place within one hour from the commencement of signs of amendment. The prognosis in these cases is very difficult, but of great importance. Such cases sometimes end, on the one hand, in true disease of the brain, terminating in paralysis and death; on the other (the purely hysterical), the symptoms pass off, and cure is effected. The case to which he referred was that of a young lady who had had two attacks. The disease passed off suddenly in both instances. The second attack lasted six months. She constantly suffered from epileptiform convulsive attacks, accompanied by refusal of food, and beating her head as if trying to commit suicide; and she also had a similar discharge of bloody mucus. This case recovered within one hour from the first symptom of recovery, and remained perfectly well, the patient describing her illness as passing off like a cloud. After being well for about a month, she fell into rapid consumption, and died. The peculiarity of these cases is the rapidity of the attack, coming on in a condition of degenerated health; then the violence of the symptoms; and, lastly, the sudden passing off of the disease. He could mention three cases in which the attack passed off within a few hours from the first commencement of recovery.

DR. FOOT mentioned a case of convulsive hysteria in a recently-seduced female, which had lately come to his knowledge, in which the most alarming symptoms were present, but in a few hours completely disappeared. He also alluded to the influence of the sexual organs in inducing these fits. From his own observation he had formed the opinion that the sexual organs had a great deal to do with the phenomena of what was popularly called hysteria; but he had also arrived at the conclusion that it was not a celibate life which necessarily induced these hysterical symptoms, and in that respect the French physician, Briquet, bore him out. Briquet ranged females, with reference to their liability to hysteria, into three classes—the completely celibate, widows, and prostitutes; and he proved that celibates were the least subject to it, that widows came next, and that prostitutes exhibited the most varied and most violent forms of hysteria. Of course, allowance had to be made for the very peculiar mental conditions which that class must often labour under. Briquet's researches went to show that it was not alone deprivation of the opportunities of gratifying sexual desires that caused hysteria. His (Dr. Foot's) own observation had certainly led him to form a decided opinion that the sexual organs were immensely connected with many of the phenomena which ordinarily go by the name of hysteria.

DR. CHARLES F. MOORE said that during some years of his life he had



seen a great number of cases of hysteria in ladies coming from India, who had suffered from tropical diseases, as well as from uterine affections. One patient was a young lady who leaped like a salmon, her heels almost touching her head. She used to spring out of her berth; had several such fits, and required to be watched lest she should injure herself. He looked on that as a case of hysteria. Another was a widow-lady coming from India, who had such well-marked cardiac symptoms that he had great difficulty in inducing her to refrain from using leeches. There was another lady also coming home, who presented an unusual symptom, viz., dislocation of the jaw. Whenever she got a fit, the lower jaw would slip out, so as to require reduction by the surgeon. Another lady complained of severe pain in the ear—so much so that a medical gentleman had been induced to leech it. She fell under his (Dr. Moore's) care when returning to India, being introduced to him by her medical attendant from London, who requested that she should be leeches if the pain came on, as it was the only treatment that gave any relief. He thought from her look that it was not a case for depletory measures, but he leeches her, to a very small extent, on the voyage between Southampton and Alexandria, and he thought she was nothing the better of it. The leeching was repeated, as stated, on the voyage between Alexandria and India, after she had passed from under his care. She afterwards died, he believed, from the account which had reached him, from the combined effects of hysteria and loss of blood, superadded to a strumous tendency of constitution, and the exhausting influence of the hot climate. Another case was that of the wife of a surgeon in India. He could detect no symptom of derangement, except a violent pain in the uterus and leucorrhœa. She pressed him to use leeches, but he declined to do so. She went to England and was leeches there, and also died, owing, he believed, to anæmia, added to the debility arising from residence in a tropical climate. It was worth noticing that in all almost these cases the patients were suffering from mental anxiety of varying degrees.

The CHAIRMAN said that the following case might, in some degree, illustrate the difficulty of diagnosis in these cases, as alluded to by Dr. MacSwiney. On Saturday morning last he was asked to see a policeman, who was brought at twelve o'clock the night before into the Whitworth Hospital, suffering from intense pain in his stomach. This pain was so severe, so agonizing, that he walked about the ward the entire night screaming with pain, and preventing the other patients in the ward from getting any sleep. No treatment seemed to have the least effect in subduing the pain. When Dr. Gordon saw him on the following morning, the first thing that struck him as remarkable was the extreme quietness of the man's pulse under such a history of agonizing pain; it

was only 76; but he was restless—incessantly tossing about; would scarcely allow of any examination of the abdomen, which was rather swollen and tympanitic; bowels confined; urine copious and light-coloured; could only swallow liquids, and everything he had taken during the entire night he had vomited, even a little water—as fast as it was swallowed it came up again; and, indeed, latterly it was quite evident that emesis took place even before the fluid reached the stomach. The diagnosis arrived at was that it was a case of hysterical vomiting, and that the pain was also of the same character; it was never referred twice to exactly the same situation—now to the epigastrium, now to the right, and again to the left side. The vomiting was also peculiar—indeed, there was no actual vomiting, but a rejection of food before it could have reached the stomach. Large doses of morphia by subcutaneous injection were directed, the result being that the man slept, got into a mild perspiration, was much relieved, and took food in small quantities. The same symptoms recurred from time to time, the intense agonizing pain, and the vomiting. That morning, when Dr. Gordon went to the hospital, he was told the man was spitting up blood. He found some blood in the spitting-cup, but on examination he was satisfied it was blood that came from his mouth or throat, and not from his lungs or stomach; that it was, in fact, induced by his own voluntary act. This, of course, confirmed the diagnosis, that it was a purely nervous affection. One thing, however, struck him as very remarkable—the rapid emaciation which the man had undergone within four days. He had endeavoured to examine very carefully into his history, but he could not find out anything which he could directly assign as the cause of the affection, except that he was a man not long in the force, had come up from the country, and had been on rather severe duty. The case bore out the extreme difficulty of diagnosis in these cases, particularly when they had to deal with a male subject.

DR. HENRY KENNEDY said that, as far as his experience went, hysteria was entirely or mainly a disease of the female sex. No doubt cases would sometimes be seen where males were affected with symptoms resembling hysteria; but he did not think anyone had ever seen the severe disease in men that occurred in females. He had seen more than one case of sudden subsidence of the disease. He recollected the case of a girl who had hysterical symptoms to a great extent, and Professor Law having got her under the influence of mercury by giving her two grains of calomel in divided doses, the symptoms instantly subsided. He would remind the meeting that a medicine brought forward a few years ago by Sir Charles Locock—bromide of potassium—had a distinct influence in these cases of hysteria. Of course impaired health added to the gravity of the symptoms in hysteria, and it was therefore

important to pay attention to the general health, and restore the tone and vigour of the system.

DR. MACSWINEY said he did not include any description of the treatment in his paper, principally because, he was sorry to say, no very beneficial result seemed to follow from the remedies he had recourse to. The first remedy which was given, and persevered in for a reasonable time, was bromide of potassium, and he must say that almost all the vaunted remedies for hysteria were one after another most carefully tried with but one result—comparative failure. No medicine administered seemed to have the slightest permanent good effect on the disease. She left hospital pretty much in the same state as she entered it. He could not say she was materially improved, but her retention in hospital became impossible, owing to the disturbance she created and the terror felt by the other patients. The impression made on his mind was that the case would terminate in insanity. They were much indebted to Dr. Foot for his remarks. The classification made by Briquet of the females most liable to hysteria was highly interesting. The Chairman had illustrated the subject by a valuable record of a case that had come under his observation. He (Dr. MacSwiney) thought he had seen hysteria in the male, and he had now under his care a young lad of seventeen, who was suffering from many of the symptoms of hysteria, though not in the aggravated form present in the case he had described.

The Society then adjourned.

# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

---

## THIRTY-SIXTH ANNUAL SESSION.

---

*Saturday, 14th February, 1874.*

EVORY KENNEDY, M.D., President, in the Chair.

*Fibrous Tumour of the Uterus.*—Dr. KIDD exhibited a specimen which he had removed from the interior of the uterus of a lady on the previous morning. So many of these tumours had been exhibited that he almost hesitated to bring the case before the Society. The tumour grew from the fundus of the uterus, and had passed through the os internum and lay in the canal of the cervix. The cervix was expanded over the tumour, and the os was so large that he could, with the tip of his finger, feel the tumour through it. The patient was an unmarried lady, forty-two years of age. She had been suffering a great deal of pain for the last two and a half years, and latterly had suffered very much from hæmorrhage. She was a patient of Dr. Ringland, and had come up from the South of Ireland and placed herself under his care, and to Dr. Ringland he was indebted for seeing the case. When he examined her, he found, in addition to the intra-uterine tumour, she had a large sub-peritoneal fibrous tumour attached to the uterus on the right side. On the left side there was another sub-peritoneal tumour, which lay to the left and front of the uterus, and then passing the finger into the rectum he found a third tumour on the posterior surface of the uterus. It was these tumours, doubtless, that gave rise to the severe pain, for he believed it was a fact that sub-peritoneal tumours were accompanied with pain, whereas intra-uterine or intra-mural tumours generally were not. The os was about the size of a four-penny piece. With a little manipulation he was able to dilate it to some extent; its margin was surrounded by a hard thin edge, which he divided with a scissors in three places, and then he was able, without any difficulty, to pass his finger round the tumour. He then passed an ecraseur in, got it round the pedicle, and divided it with great ease. The only difficulty in the operation was getting this mass out of the uterus. The pedicle was small and was cut off close to the surface. He had to seize the tumour with a vulsellum and to pass in a tenaculum and roll it out of the uterus.

*Specimens of Mole Pregnancy and of so-called Uterine Hydatids.*—Dr. T. MORE MADDEN said the cases to which he wished to direct the attention of



the Society were of interest, on account of the comparative rarity of their occurrence, as well as from the difficulty of their diagnosis, which might implicate the character of the individual who was affected with the disease that gave rise to these productions. Some six or seven years ago he published a paper in the *Dublin Journal* on Ovarian Hydatidiform Disease. He had then seen but two cases of the kind. That the disease was of rare occurrence was shown by the fact that, in the Reports of some of the Masters of the Rotunda Hospital, but two or three cases were mentioned; and Dr. M'Clintock, after an experience of ten years in that hospital as Master and Assistant, mentioned but ten cases, in his work on the Diseases of Women, as having come under his observation. He thought, therefore, in the case of a disease of such rarity, it was the duty of every member of the Society, who had seen anything bearing on the question, to call attention to it. The first case he had to bring under their notice was that of a woman aged twenty-five. She had had two children, the younger of whom was between two and three years of age. At the time he saw her she imagined herself to be in the fifth month of her third pregnancy. She lived a short distance from town, and had been threatened with symptoms of abortion before he saw her. He found, on reaching her house, that the flooding was rather profuse. The os was about the size of a florin, very soft and dilatable, and he could only make out that there was a soft mass inside. About an hour after he came there was a smart dash of hæmorrhage, and a large quantity of these so-called hydatids was expelled. They, to a considerable extent, filled the chamber utensil, and he collected some of them, which he now exhibited. They were of the ordinary character of the growth, varying in size from that of a small pea to that of a small grape. After their expulsion the hæmorrhage ceased, the uterus contracted, and the woman made a good recovery. The second case which he had to bring under their notice was an example of mole pregnancy. A woman came to him four months ago, telling him she believed herself to have become pregnant thirteen months before. She had had seven or eight children, the youngest of whom was six years old, was approaching her fortieth year, when the symptoms of pregnancy again presented themselves. Menstruation ceased, she commenced to suffer from sickness in the morning, her breasts enlarged, and at the usual time she declared she felt quickening. At the sixth month she fell down stairs, and the motion of the child, which she said she had felt, suddenly ceased, her abdomen became flat, her breasts became smaller, and she expected a miscarriage. Nothing, however, took place, and she remained in the same condition until the time she consulted him. On examination he found no sign of pregnancy. The uterus was a little enlarged, the breasts were quite flat, and the abdomen was not perceptibly enlarged. It occurred to him that there might be some disease of the uterus, and he introduced the sound and

found the uterus larger than natural. The following morning when he came to see her she said she was in labour. She had had a good deal of hæmorrhage and violent pains, and shortly after he arrived the specimen he now exhibited was expelled from the uterus. It was an example of so-called mole pregnancy. It was hollow, about the size of a small pear, was rather more than half an inch in thickness, and corresponded very exactly with the enlargement of the uterus. All the symptoms subsided, and at the end of a month the woman had her usual changes, and had since been regular. The case he had first described was of interest in one or two points. In the first place, there was the question, whether these hydatidiform masses were the products of conception? The usual opinion was that they were the abnormal growth of the chorion villi of a blighted fœtus. Others held that they were placental growths; and others were of opinion that they were not always necessarily connected with pregnancy. He himself held the latter view. Cases were on record where women of unimpeachable chastity had expelled these hydatidiform substances from the uterus.

The theory that hydatiform moles of the uterus are the result of cystic disease of the chorion, or the abnormal growth of the degenerated chorionvilli of a blighted fœtus, although applicable to the cases which Dr. More Madden had, himself, observed of this disease, was, in his opinion, clearly disproved as being the sole cause of these substances, by the numerous cases on record in which so-called uterine hydatids were expelled under circumstances excluding the probability of pregnancy. And he still adhered to the views he had formerly put forward to account for this fact, namely, that these exceptional instances might be accounted for by ovarian disease implicating one of the graafian vesicles discharged from the ovary at each menstrual period, and of the growth of this diseased vesicle after its escape into the uterus, until its increased bulk is such as to excite uterine irritation and expulsive action.

In one case which came under his observation the woman supposed herself to be seven months pregnant, and a small fœtus of three months, perfectly formed, was expelled with a large mass of hydatids. In that case, no doubt, the hydatidiform mass was the result of the death of the fœtus. He believed, however, as he just observed, that these hydatids might be developed in a woman without pregnancy.

The PRESIDENT asked whether Dr. Madden had to resort to any operation to remove the "mole" growth?

DR. MADDEN replied that it was expelled by uterine action after a severe pain, and without any interference on his part.

The PRESIDENT said it appeared to him that they had been dealing with

two distinct forms of disease—the hydatid and the mole. In his own experience he did not recollect any case of hydatid growth that was not the result of the degeneration of the ovum after impregnation; and in all the cases he had seen the hydatids were, so far, interesting, as they were a generation, *per se*, superadded to the original ovuline growth—a gemmiparous generation that multiplies itself by division, a portion of the growth becoming detached, and thus forming another creature or succession of creatures by prolongation and contraction. The most perfect specimens of gemmiparous generation we have are to be seen in hydatids. The mole is a different structure. It appeared to be something more of intra-uterine polypoid growth projecting from the inner structure of the uterine wall, or originating in an effusion of blood into the decidual cavity or chorion cells, and eventually detaching itself from the place from which it sprang. He had met with some of these moles, in which it was necessary to detach a portion in order to remove them; but it was always a solid structure, and consisted of one mass. It seemed a polypoid growth, that detached itself from the uterus by absorption, and lay as a free growth within it.

Dr. KIDD should like to ask the President whether he had himself made any observation, or was aware of any recent observations that proved these peculiar growths to be true hydatids? So far as his knowledge went, and he had examined them microscopically—and they had been examined by many much more experienced microscopists—they are, at the present day, universally believed not to be hydatids at all; and all our best pathologists recommend that they should be called hydatidoid or hydatidiform growths. The disease found in the brain and liver, for instance, presented the true hydatid. These were distinct animals which grew in the form of a cyst, and the whole history and structure of them had been clearly made out. They were distinct animals which went through a whole series of changes, being taken in food in one form, and going through a whole series of growths or developments, which ultimately resulted in true hydatids, or acephalocysts; but no such history attaches to the specimen before the Society, and he thought it important to observe the distinction between these growths and true hydatids which grow by gemmation.

The PRESIDENT said that some five-and-twenty years ago he read a paper before the Royal Irish Academy, in which he satisfied the Academy and himself upon the question of the gemmiparous growth of the hydatids. There were two distinctive growths in hydatids—those that were detached and those that were attached. The attached hydatid was that which they were familiar with as an ovarian growth, but there was no doubt the true hydatid was detached and was capable of an indepen-

dent existence. In many of the hydatid growths, on the other hand, they grew by gemmiparous division, after contraction and narrowing occurred, but they did not become detached, and it was very rare in uterine hydatids to find them detached.

MR. F. T. PORTER asked if Dr. Madden had submitted any of these hydatids to a microscopic examination?

DR. MADDEN replied that he had not done so because the question as to the nature of these growths had been investigated by so many histologists, with the assistance of the microscope, that he considered it quite useless for him to endeavour to throw any light on it in this way. There were different ideas as to what caused them. One idea was that they were the products of conception; another was that they were not the products of conception but of ovarian disease; and a third was that they were of the same character as the hydatids found in the brain and liver of sheep. The last theory he had not referred to, as though some cases of true hydatids in the substance of the uterus are reported, yet, as a cause of hydatidiform mole, the idea of these being produced by independent animal organizations, or acephalocysts, as supposed by many of the older writers, seemed to have been exploded long ago.

DR. M'CLINTOCK said if they entered into a general discussion on the question of hydatids, it might occupy them a long time. He must concur, in a great measure, with the remarks Dr. Kidd had made, and reiterate what he (Dr. M'Clintock) had said before, that the title "hydatid of the uterus" was a double misnomer. The disease was not hydatids, and it was not a disease of the uterus. In all the cases he had seen it had been the true vesicular disease of the chorion, and there was a complete absence of echinococci. It was not a disease of the uterus, but of the ovum. A more practical and important question connected with this affection was a medico-ethical one—namely, how far these morbid products were to be regarded as evidence of pregnancy having occurred? Of course there were cases where, if the practitioner announced that the expulsion of these substances was a proof of conception, it would amount to a very serious imputation on the character of the patient. When, therefore, they came from an unmarried woman, a man ought to be cautious in expressing his opinion. In no case that fell under his observation was there any ground to doubt that the morbid growth was the result of impregnation. He was very much inclined to doubt, notwithstanding all that had been written on the subject, whether there was any clear indisputable case on record to prove that such a disease as this had existed in a virgin. It was possible that a woman nine or more months after she became a widow might discharge a



hydatidiform product, without necessarily incurring a doubt as to her purity; for, as Dr. Madden had shown, it might be retained in the uterus beyond the period of uterine gestation. He did not deny that true hydatids might occur in the uterus, though he believed the occurrence of true hydatids in a cavity lined with a mucous membrane was extremely rare. He did not think they were ever found in the intestinal canal or in the bladder. He, therefore, did not think analogy confirmed the possibility of the formation in the uterus of a true hydatid growth. At the same time he should be unwilling to say that hydatid growths could not be found in the uterus of an unmarried woman.

DR. HENRY KENNEDY said he had once seen a case in which there had been a large discharge of these hydatidiform products, and the patient subsequently lost her life from puerperal fever—blood-poisoning.

DR. MORE MADDEN said the President had properly called attention to the fact that the two diseases which he had brought under their notice were dissimilar; and as that might appear to imply that he (Dr. Madden) made no distinction between them, he begged to direct attention to the title of his paper, in which the distinction was clearly marked, viz., “to exhibit specimens of Mole Pregnancy, and of so-called ‘Uterine Hydatids.’” With regard to the question of the possibility of the latter occurring in an unmarried female under circumstances that precluded the possibility of pregnancy, he thought there were cases on record which should satisfy them on that point. Dr. Ashley, who had written on the subject, recorded a case where a woman, who had been a sufficient time in prison to prevent the possibility of pregnancy taking place, or, at least, to render it in the highest degree improbable, produced these growths in the uterus. The celebrated Dr. Cullen made a mistake in a case of this kind. Dr. Hamilton, of Edinburgh, was absent, and Dr. Cullen was called in to see one of his patients. He pronounced the case to be one of miscarriage. This woman had been separated from her husband for two years, and she lost her social position in consequence of Dr. Cullen’s opinion, was turned out by her family, and became an outcast. The substance expelled from the uterus was kept, and when Dr. Hamilton returned to Edinburgh he examined the mass, and pronounced it to be a case of uterine hydatids, and expressed his opinion that it was not the result of conception. He merely mentioned these cases, as the question was one of considerable interest. Another instance was on record of a female prisoner giving birth to a living child, and some months afterwards expelling hydatids—she being placed in circumstances where conception could not take place.

The PRESIDENT could confirm Dr. Madden’s statement so far as this, that he had seen, after a natural birth, an hydatidiform change on the

placenta, which left no doubt that it had undergone the change which produced hydatidiform growths.

*A Case of Hysterical Convulsions, with some Remarks on Amenorrhœa.* By  
F. T. PORTER, L.R.C.S.I.

It is to be regretted that the subject of the nervous disorders of females has not attracted that amount of attention which has been extended to other more striking, but not less useful, questions.

I, accordingly, consider that a class of cases which contribute largely to the every-day responsibility of the practitioner ought to receive, at the hands of thinkers on medicine, the same consideration which has been devoted to questions of a more extraordinary, but comparatively unusual, nature.

The case which I am about to lay before the Society is one of hysterical convulsions. I also purpose entering into the consideration of the treatment of amenorrhœa, at the same time restricting my observations to the purely functional forms of the affection.

The case, which furnishes a remarkable instance of the effect of emotion in the causation of convulsive disorder, came under my notice on April 20th, 1873, on which occasion I found the patient, an unmarried girl, now in her twenty-seventh year, in a convulsive fit, which was followed by religious delirium and sobbing. She put me in possession of the following history of her case:—That she began to menstruate at thirteen years of age, and that she had enjoyed the best of health up to her seventeenth year, at which time her father died very suddenly. The intelligence of her father's death caused such a shock to her system as to induce convulsions. The convulsions were very frequent—three attacks often occurring in the course of a day. The menses stopped in October, 1872, but returned on 15th of last October. During the interval of the stoppage of the menses the fits became less frequent, but more prolonged. She entered a Surgical Hospital early in April, 1873, but remained only a fortnight under treatment. When in hospital she had been treated with bromide of potassium, which I learned from a prescription produced. She presented an emaciated and anæmic aspect, and complained of loss of appetite, lowness of spirits, headache, spinal tenderness, and constipation. I found her suffering from the combined effects of hysteria, amenorrhœa, bromism, and strong tea. She stated that for four years she was too delicate to follow her avocation (that of a laundress). I commenced treatment by prescribing an aperient draught. I applied a blister to the nape of the neck. The headache was relieved by these measures. I followed up my treatment by the exhibition of valerianate of zinc in 5 gr. doses twice a day for a fortnight. The convulsions yielded like magic to this remedy. I then administered 2 gr. doses of extract of hemlock twice

a day, and continued its use for a fortnight also. I consider hemlock to exercise considerable power in nourishing the nervous centres. I do not, however, believe that large doses of the drug are necessary in the treatment of hysterical patients. At the end of a month from the commencement of my treatment the patient had had only one slight attack of convulsions, and has had no attack since. She is now well, and is able to follow her avocation, and suffers no inconvenience, except slight nervous excitement when menstruating. The case is interesting as demonstrating the effects of emotion; and the fact of a girl menstruating in her thirteenth year is not usual in this climate. It is also strange that the convulsions should have decreased during the period of the stoppage of the menses.

I consider hysteria to be a most unsuitable expression for a group of disorders by no means confined to one sex. The epoch of puberty bears a strong resemblance to that of dentition—in both there is an increased development of the nervous centres and a specialized evolution of nervous force. The so-called hysteria is referable to the increased nervous activity which, during puberty, is common to both sexes.

Practitioners are not alive to the advantage of observing the phenomena of puberty. It is probable that, owing to nervous disturbance, as many organic diseases are induced during the accession of puberty as there are during that of dentition.

I have not much faith in the drug-treatment of an emotional disorder like hysteria; but I prefer the valerianates, hemlock and lupulus, to the bromides. I consider the bromides to act most injuriously in hysterical cases. Their exhibition tends to derange digestion, to deprave the blood, to weaken the heart, and to retard menstruation. The devotion with which many practitioners adhere to the use of the bromides is a melancholy instance of the evil effects of fashion in medicine. When spinal tenderness co-exists with hysteria, I generally employ Corrigan's iron with considerable success. Much depends on the proper regulation of a patient's habits. Temperate meals, early rising, cold bathing, and active exercise in the open air are indispensable elements of treatment. The treatment is more moral than medical. The morbid excitability of the emotions, so common at the present time, is a fact patent to every observer; and the influences in this respect of sensational literature, long engagements, and a host of other social evils, ought not to be ignored. An ancient sage stated that all disease proceeds from the mind, and this is fully exemplified in the case of hysterical persons. Many writers consider the unmarried to be more liable to hysteria than the married; but, so far as my humble experience enables me to form an opinion, the reverse is the case. The most aggravated cases of hysteria I have had to treat occurred in married women. Family cares, pecuniary anxieties, prolonged lactation, and other causes, incident to married life, act as

injuriously on the nervous system as any evils imputed to celibacy. Before alluding to amenorrhœa, I purpose eliciting a few observations on the nature of menstruation. Menstruation corresponds to the period of "rut" in the lower animals. The question naturally arises—why is the period of "rut" not accompanied by a sanguineous discharge, as is the case with menstruation? The theory that the menstrual discharge is surplus blood is a mere assumption. Dr. Ramsbotham looks on the discharge to be the rudiments of the deciduous membrane; but why, may I ask, is the discharge absent in all the deciduous mammals below the human female? The fact of the absence of this sanguineous discharge in the lower animals, coupled with the fact that it is scanty in women in the savage state, has induced me to form the opinion that its existence is, in a great measure, due to causes incident to the long-continued effects of civilization. It is to be regretted that the question of the final cause of menstruation has not been elucidated—it is a question pregnant with physiological interest.

I am anxious to elicit opinions as to the age most favourable to the healthful accession of menstruation. Some practitioners would have it that a girl should begin to menstruate at fourteen or fifteen; others, again, would fix seventeen as the limit. I am inclined to agree with Mr. Robertson, of Manchester, and consider a later period to be more in unison with the intentions of nature. Some authorities base their opinions on the observation of cases which, owing to the enervation incident to civic life and a highly artificial state of society, are but badly calculated to furnish materials for forming any sound conclusion on the question. It is strange to assume that the generative function should come into play before the nutritive processes so essential to the welfare of the individual have been perfectly consolidated. Such an assumption is opposed to the spirit of all sound physiological reasoning. There can be no more fertile cause of delicacy than the premature approach of menstruation. Such an event often engenders disease by drawing off the vascular and nervous energy so essential to the consolidation of the functions of nutrition and growth. The premature accession of menstruation is certain to be followed by the early disappearance of the function. The immediate cause of functional amenorrhœa is, I conceive, an inability of the nervous centres to stimulate the ovaries. This inability may be owing to the retention of excreta in the blood. The suppression which often follows renal congestion after scarlatina will serve as an example of this cause. It may result from too little vascular pressure, as in anæmia, or too great pressure, as in plethora. It is on the two latter causes I wish more particularly to dwell. In treating these conditions, practitioners neglect to bear in mind the influence of the sympathetic system on the blood-vessels, and they generally address their treatment to the blood itself. In plethora the



sympathetic system is depressed. This is evidenced by the increased animal heat, contracted pupil, and vascular relaxation. I consider that in such cases belladonna is a most efficacious remedy. It has been used with success on the Continent, but I am not aware of any practitioners who prescribe it in this country for amenorrhœa. I have often used it in my own practice with considerable success. The late Dr. Graves used belladonna to relieve the cerebral congestion of typhus. It was that circumstance which induced me to employ it in the treatment of plethoric amenorrhœa. In anæmia the sympathetic system is in a state of tension, which is evidenced by the dilated pupil and diminished animal heat, and in such cases I generally administer small doses of opium before resorting to the ordinary remedies. Hemlock is beneficial when opium cannot be borne. It is probable that the good effects of hemlock in splenic tumours are owing to its effect on the innervation of the smaller vessels. Anæmia, like plethora, is not, I conceive, so much an alteration in the condition of the blood, as it is an alteration in the innervation of the blood-vessels themselves. It is not my intention to touch on the local causes or treatment of amenorrhœa. I will not notice the subject further than to say that local conditions, as a rule, depend on constitutional causes, and that consequently (but especially in the unmarried) all means of a constitutional nature should be resorted to before local measures are adopted.

*Rupture of the Urethra and Perinæum during Labour.*

By S. M. MACSWINEY, M.D., &c.

June 1, 1873, I visited Mrs. D., Phibsboro', stated to have been delivered at full term, of a still-child, twenty-four hours previously. I obtained, on arrival, the following history:—She was twenty-four years of age, and two years married. Some months after marriage her health became impaired, and she was under medical treatment for several weeks. Ten months after marriage she was delivered of twins, still-born. They were, it was calculated, of seven months uterine age. Since then she has been in fairly good health. She was attended in her recent confinement by a most experienced and respectable midwife, who tells me that she was called to Mrs. D., at six, a.m., on Saturday, May 31st. Mrs. D., said she had had pains from about two a.m. of same morning. Labour proceeded very regularly and passed steadily into the final stage without any unusual symptoms. At eleven, a.m., the child was born; slight traction was soon after made upon the cord when it gave way, separating from the placenta. The case now became one of retained placenta, with which the midwife would not interfere. The husband was despatched for me, and, not finding me at home, brought Dr. Cranny, assistant physician, Rotunda Hospital, with him. When Dr. Cranny arrived, the placenta had

already been expelled, and he did not, therefore, interfere in the case. The nurse was aware that there had occurred a rupture of the perinæum of the woman at the moment that the body of the child had been born, but she thought it was slight, and did not then notice it particularly, further than by enjoining more than usual quietness on part of the patient until her next visit. She accordingly visited early on the morning of June 1st, and learning from Mrs. D., that her water was every minute coming away from her, she examined her more minutely. The result was that she ascertained the existence of a very extensive tear in the perinæum, and immediately called for medical advice. So much for the previous "history" and account of the last labour of Mrs. D., as given, by this very intelligent nurse, to me on my arrival. I found her "present state" to be as follows:—Mrs. D., was a cheerful, healthy-looking young woman; her pulse was quiet; she had slept well, and she was free from pain or any symptom of fever. She had, she said, no complaint to make but "that her water was coming away from her" and keeping her in constant wet. Questioning her minutely respecting her condition before her labour, I learned that there had been some difficulty in making water for the last eight or ten days; that it would come regularly enough and in sufficient quantity, at one time, scantily and with a certain amount of distress at another. She had tried to make water, she said, at ten, p.m., on the night preceding the morning when her labour commenced, but failed to discharge more than a few teaspoonfuls. When the nurse saw her, at six a.m., she inquired about the water, and caused her to make an effort to pass some at once, but she did not succeed then. After the lapse of some couple of hours, however, she—when endeavouring to urinate—was noticed to void a considerable quantity of water into the vessel. The nurse remarked to her that this was her water, and the patient herself says it was the contents of her bladder she passed at this time, and that the only thing remarkable about it was, that it poured away in an unusually abundant stream. I found, upon examination, the following state of parts:—The vulval opening extended back to, and was continuous with the anal. It was occupied by a quantity of very fluid, brownish-red coloured discharge, having an urinous odour. A catheter passed through the urethral orifice, in the ordinary way, made its appearance in the vagina, at a point about two inches from the external meatus. Following up this examination, a finger, cautiously introduced for further exploration, enabled me to ascertain that there was a complete transverse tear of the vagina, and division of the urethra. The parts were much swollen and jagged, and the urine flowed, in constant drops, into the vagina. This poor lady was thus found by me, twenty-four hours after her confinement, suffering from a ruptured perinæum, extending fully into the rectum, whose sphincters were torn across, and a urethro-vaginal tear of great extent. The urethra, two inches from the external meatus, had

been torn across; the corresponding portion of the vagina had been similarly lacerated; the urethral wall protruded through the fistula, and formed a small, scarlet-coloured tumour. Naturally, the inquiry arose at once to my thoughts, "How has this dreadful injury occurred?" Now, it is certain that the laceration was not inflicted by the use of obstetric instruments, for none such were had recourse to; neither was the lesion brought about in the manner in which a vesico- (or urethro-) vaginal fistula is ordinarily produced, for there had been no prolonged pressure of the head upon the neck of the bladder or urethra—the labour having been completed within the normal period of a natural confinement, and the injury existed immediately after the birth of the child. When the experienced midwife, who attended this lady, found that the placenta was retained, she made firm pressure over the uterus until the after-birth was expelled, but she did not even once introduce her hand into the vagina, and I feel sure that no amount of pressure upon the abdominal parietes over the uterus (even supposing, what is highly improbable in this instance, that it was excessive) would be at all capable of producing the lesion which actually existed. An ulcer on the vaginal wall, immediately over the urethra, might have extended so deeply as to cause the canal to be laid open by the pressure of the body of the child in its passage into the world; and there exist reasons for concluding that there had been some unhealthy action taking place in this region, for some time previously. Still I fear that this, although a possible, is a somewhat far-fetched and improbable hypothesis, and, on the whole, I think, that the most reasonable conclusion to arrive at is, that the bladder contained a large amount of water at the moment of the birth of the child; that a sudden, and unusually powerful expulsive contraction occurred which swept the uterine contents beyond the vaginal outlet before the parts were sufficiently dilated, and when, consequently, they gave way at their weakest points. This explanation may not be deemed satisfactory, and possibly is not the correct one, but I am unable to form any other opinion, from the circumstances as they are known to me. Dr. Kidd saw this lady, with me, two or three times. He made a very careful and minute examination of the injured parts, and regarded it as a very remarkable and rare example of injury. I watched the progress of the case with much interest. There occurred a good deal of sloughing of the vaginal mucous membrane, and the aspect of the parts was very unsatisfactory for ten or twelve days. After this time, however, the appearances greatly improved; there was no "scalding" or pain; her general health and strength were excellent, and she thought, she told me, that the *constant* flow had ceased. She could now retain a wineglassful or more of water, which, however, came away when she changed her position, or stood erect. I ceased being in attendance after about six weeks, but have seen her three or four times since. The last time I saw her was

about four months ago. At that time I made the following note :—"The constant dribbling of the water has ceased; she can hold half a tumblerful of urine in the bladder, and discharge it by her will; she is now rarely wet by its coming away involuntarily, and this only occurs when she keeps it too long. The complete division of the urethra, at the time of the accident, seems to have permitted of an attempt at a curative effort, much more so than if it were only partially opened, as occurs in ordinary fistulæ. There appears to be a new development of the muscular fibres of the urethra, which acts as a sphincter, and is sufficient to arrest considerably the involuntary flow of urine. The anterior (or outer) extremity, of the divided urethra is occupied, at the point of division, by a scarlet-coloured, protruded tumour, whilst the internal (or posterior) divided surface is considerably retracted and hidden away near the os uteri, and its orifice, leading into the bladder, difficult to be seen, or 'hit' by the catheter. She has wonderful control of the rectum, and suffers, she says, extremely little inconvenience from the injury there." Since then I have not seen the lady, and, therefore, I know not whether she is content to await patiently the slow restoration to comparative health which at that time was undoubtedly in progress, or has sought other advice with a view, perhaps, to operative action.<sup>a</sup>

DR. LOMBE ATTHILL asked was the anterior portion of the urethra permeable?

DR. MACSWINEY said it was, but latterly the water seemed to come away from the neighbourhood of the os uteri.

DR. KIDD said that Dr. MacSwiney had been kind enough to allow him an opportunity of seeing this patient, and he had very accurately and carefully described the condition they found her in on the occasion of their first visit. She was confined on Saturday morning, and he saw her on Sunday afternoon. There was at that time a complete laceration of the perineum; the whole of the vagina was very much swollen; it was sloughy and soft, and the water was constantly dribbling away.

<sup>a</sup> The subject of ruptures and injuries of the genital tract has been fully and ably treated by Dr. M'Clintock, whose paper on "Laceration of the Vagina, in the course of Labour" (*Dublin Quarterly Journal of Medical Science*, May, 1866), may be referred to by all interested in the subject with advantage. This eminent physician was not acquainted, at the time when his paper was written, with the history of any case in which "the lower portion of the anterior wall" of the vaginal canal was torn. But he cites a case, recorded by Drs. Johnston and Sinclair (No. 10, p. 344 of their book), in which there was a transverse slit of the base of the bladder, just at the point of relation to the vagina, which is a near approach to a rent in the lower portion of the anterior vaginal wall. Dr. M'Clintock likewise draws attention to Dr. M'Keever's Essay—"Practical Remarks on Laceration of the Uterus and the Vagina: London, 1824"—whose case (No. 1, p. 44 of the Essay) is an example of injury of the bladder in anterior laceration of the vaginal wall.



There was one point in which he would supplement Dr. MacSwiney's statement, and that was as to the size of the opening into the bladder. It was so large that he passed his three fingers into it, and without using force he could have easily passed in his four fingers. The woman had no constitutional symptoms of suffering at the time; her pulse was quiet, her skin cool; she was placid, made little complaint, and, in fact, was one of the easiest-tempered women he had ever seen. He saw her about a week afterwards, and also a third time at the expiration of a few months. At that time she stated she had perfect control over the bladder; could retain water, and pass it at will. He passed a uterine sound through the urethra, and was able to bring fully into view the extremity of the divided urethra. He could see that the urethra had been completely torn across, the laceration being so complete that the urethra and the bladder were perfectly detached from each other. The opening into the bladder had a puckered feel, as if the cicatrix had undergone contraction, and the parts had closed in. When he first saw her, he and Dr. MacSwiney examined and cross-examined the nurse in attendance very carefully as to the facts of the labour, and to his mind it was satisfactorily established that no instrument was made use of, and no violence used to which the laceration could be attributed. The whole of the second stage of labour was completed in a remarkably short period. There was only one further point to which he would refer, and that was as to the explanation of this occurrence. Dr. MacSwiney had suggested the idea that there was prolapsus of the bladder with water in it, and that it was caught between the pubes and the head of the child; and that the child being rapidly and forcibly driven out, caused this prolapsed portion to be ruptured. At first that seemed to him a very probable explanation, but on considering it afterwards, and remembering that the whole of the urethra was torn, he doubted whether that explanation was satisfactory. He was diffident in offering a suggestion. Cases of labour have occurred where the whole circumference of the vagina was torn by uterine action before the birth of the child, so as to completely detach the uterus, and allow it to come away with the placenta. Dr. Braxton Hicks has explained the mechanism of this. His idea is that the exit of the head being opposed, the uterus, as it could not expel the child, drew itself up, or glided up on the child, until it tore itself away from the vagina and all its other connexions. Dr. Kidd was inclined to think the same explanation would apply in this case. This woman had softened tissues. The exit of the head was opposed by the perineum; a violent pain occurred; the uterus drew itself up off the child, drawing up the anterior wall of the vagina with it, and in that way caused the laceration of this portion of the vagina of the urethra. At this moment the perineum gave way, and so saved the remainder of the vagina, and permitted the exit of the head. The woman described the pain with which the same

head was expelled as a very violent one. The recovery in such a case was most remarkable.

The PRESIDENT.—Then you attribute the rupture to somewhat the same cause as gives rise to rupture of the tendo-Achillis, or of the patella.

DR. M'CLINTOCK thought the case detailed by Dr. MacSwiney, with such remarkable perspicuity, was almost an unique one. He had met with, and carefully examined *post-mortem*, many cases of rupture of the vagina and rupture of the uterus, and rupture of both; but in all those cases where the vagina was ruptured, it was almost always the posterior and lateral part that gave way, and the tear always took a somewhat longitudinal direction. In this case, however, the laceration was in front. They all knew that the lower part of the uterus was liable to laceration under the efforts of parturition, quite independently of the use of any instrument, and that this might extend and involve the lowest segment of the vagina, they were well aware. It would, *à priori*, appear not an improbable thing that a laceration of this kind might take place in front, but he had never seen such a case. The reparative power of nature in the case detailed was very remarkably exemplified—for, notwithstanding the rent into the bladder was so large as to admit of the introduction of three fingers, matters had so changed as to enable the woman to have some control over her bladder.

DR. MACSWINEY said he had asked Dr. Kidd to see the patient, with a view to obtain his assistance in operative proceedings, and it was with that object he had instituted the exceedingly minute and careful examination which he had made, with the result of coming to the conclusion that operative interference at that time would be unadvisable. The only point as to which Dr. MacSwiney did not satisfy himself was the extent of the opening into the bladder. He was unwilling to go backward so far with his finger, but Dr. Kidd having made the examination, set that point at rest. There was not only this great injury to the bladder and the urethra, but the perineum was laid completely open, and for some time the contents of the bowel passed away without the control of the patient, and in her bed. It was the combination of two great injuries that made the case remarkable, in addition to their occurrence in labour. It appeared to him that, just as in an accidental wound of an artery, where the curative measure was division of the artery, something like that would take place in the urethra where it was partially opened, if it were divided altogether. Such a case was on record—a case described by an English surgeon of an injury to the urethra, in which there was an artificial sphincter formed at the vesicular end to prevent the trickling of the urine.

The Society then adjourned.

## PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. LYONS, President.

DR. BENNETT, Secretary.

*Ulceration of the Ileum in Enteric Fever; Successive Hæmorrhages; Death on the fifteenth day.*—DR. A. W. FOOT exhibited a portion of the lower end of the ileum of a lad, aged sixteen, who had been brought to the Meath Hospital on the ninth day after the initial rigour of his fever. He had several discharges of blood from the bowels on the day of admission. He exhibited three bad symptoms in addition to the hæmorrhage—a lethargic apathetic condition, rigidity of the large joints of the upper extremities, and that condition of the tongue in which, after protrusion, it is left outside of, or between, the teeth till it is put back into the mouth; he had also twitching of the hands and morning delirium. The hæmorrhages were almost of daily occurrence. He died during, or immediately after, a very profuse loss of blood, at 10 p.m., on the sixteenth day.

The average temperature (deduced from fourteen observations made twice daily, morning and evening) was  $103\cdot5^{\circ}$  Fahr., the max. temperature  $105\cdot1^{\circ}$  Fahr.; the min. temperature  $101\cdot8^{\circ}$  Fahr. The average pulse was 112·8, the max. 126, the min. 100. The average rate of respiration was 32·7, the max. 60, the min. 24.

The body, examined twelve hours after death, still retained some warmth (month of September); rigor mortis well marked; blood exuded from the anus on turning the corpse. No evidence of recent peritonitis within the abdomen. The last three feet of the ileum presented a series of ulcerations increasing in number and size towards the termination of the small intestine. The bowel, for eight inches above the ileo-cæcal valve, also the cæcum, and the ascending colon, were filled with blood, some clotted and some liquid, of a port wine colour; the mucous membrane was extensively stained of a crimson colour from imbibition. Some thin claret-coloured fluid lay in the depth of the pelvis, probably the result of *post-mortem* dialysis. About six inches above the ileo-cæcal valve was an oval mass of ulceration, which seemed most likely to have been the principal source of the hæmorrhages, as it appeared to be in the most advanced stage, and clotted blood covered its surface. The ulcers were raised, thick, rugged, fissured, covered with ochreous-coloured sloughs. The ulcerative process commenced as small round isolated ulcers, occupying the centre of shaved-beard patches of the agminated

glands, about three feet from the termination of the ileum, and increased rapidly in number and size towards the ileo-cæcal valve, just above which they formed a confluent sheet of conglomerated ulceration. There was no glandular affection obvious in the cæcum or colon, but in the diseased part of the ileum the solitary glands were like poppy seeds strewn over the surface of the bowel. The mesenteric glands, especially those connected with the end of the ileum, were enlarged, buncy, succulent. The large and soft spleen weighed 18 ozs., and measured 7 inches by 8. Although the death was one from hæmorrhage, hypostatic congestion of the skin of the back was sufficiently marked to be quite obvious, though by no means of the degree generally observed in death from enteric fever without hæmorrhage. The posterior parts of the lungs were also much congested, and contrasted strongly with their white, fleecy, anæmic anterior parts.—  
December 13, 1873.

*Perforation of the Ileum in Enteric Fever.*—*Extravasation of the Intestinal Contents.*—DR. A. W. FOOT exhibited a portion of the lower end of the small intestine of a man, a boot maker, aged forty, who had died on the sixth day after the appearance of symptoms of acute peritonitis, and on the sixteenth day after the initial rigour of the fever. The acute symptoms occurred suddenly, at 3 a.m., on the tenth day of his illness, immediately after an evacuation from his bowels; they were severe pain, referred originally to the right iliac fossa, but soon extending upwards towards the epigastrium, and thence all over the abdomen, vomiting of green bile, and collapse. He had been lying ill in his own house, suffering from prostration, ochreous diarrhœa, thirst, loss of appetite, and nocturnal delirium. He recently had been much depressed in mind and body owing to a long and anxious attendance upon his wife, who had just died of phthisis. Upon the afternoon of the day on which the symptoms of peritonitis appeared, he walked, with the assistance of a friend, to the hospital, a distance of a quarter of a mile. When he arrived there his temp. was 97° Fahr. and pulse 86, small and weak. He was at once given a dose of Battley, and means were employed to procure reaction. The sensitiveness and tenderness of the entire abdomen were extreme, but they yielded in a few hours to opium, and light poultices of linseed meal. He had two or three small stools subsequent to the occurrence of the sudden pain, and before his admission to hospital. These probably were the contents of the colon; there was no motion from his bowels after his admission; there was no urine passed for twenty-four hours after his admission, and that then voided was scanty, high coloured, and loaded with urates; it did not contain albumen. He had thirty grains of watery extract of opium during the five whole days he was under observation. The opium relieved the pain so much that on the third day he could quite tolerate manual examination of the abdomen; it did not affect his head, which



remained quite clear all along. Hiccough came on on the third day, causing distress from the pain it produced, and about the same time twitching of the hands. The green vomiting continued, with but slight intermission. On the day after admission, three rose-coloured spots were found on the trunk. His thirst was satisfied and the vomiting lessened by sucking ice; beef tea and iced milk, in small quantities, remained on the stomach. The highest point of temperature was  $100.8^{\circ}$  on the night before his death, which occurred on the morning of the sixth day after the development of the acute symptoms. During life there was but moderate tympany of the abdomen. He lay on his back with knees drawn up; his face was pale, haggard, and anxious. At 2 p.m. on the fifth day (fifteenth of the fever) he got a fresh attack of violent pain in the abdomen, which, after some abatement, returned with fresh violence at 2 a.m. on the sixth day, and in six hours afterwards he died. His average temperature, from twelve observations upon it, was  $99.5^{\circ}$  Fahr. the max. temp.  $100.8^{\circ}$  Fahr.; the min. temp.  $97^{\circ}$  Fahr; (the observations were made twice daily, morning and evening). The average pulse was 104.7, the max. 116, the min. 86. The average rate of respiration was 23.4, ranging between 16 and 32.

The body was examined before it was cold. There was no peritoneal tympany. The moderate tumefaction of the abdomen, which still persisted, was due to distension of the jejunum and upper parts of the ileum, with gas and fluid fæces. The contents of the abdominal cavity were bathed in bright yellow liquid, which after the most careful and gentle separation of the agglutinated intestinal coils, was found to issue from the orifices in the floors of two Peyerian ulcers. The perforations were detected by observing the issue of a gentle stream of water passed through the bowel. The ulcerations were comparatively few and of small size, and were confined to the last twenty-four inches of the ileum, in which eight separate ulcers were observed, in addition to an irregular confluent mass immediately above the ileo-cæcal valve. There was a remarkable absence of ochreous sloughs about these ulcers, which, though not of large size were deeply excavated. The extravasation had taken place through the frayed peritoneum, which formed a treacherous floor to two of them. There was a notable absence of any obvious enlargement of the solitary glands in or about the diseased part of the ileum, and none were observed in the cæcum. The usual appearances of intense and general peritonitis were present. The quantity of bright yellow fluid in the abdominal cavity might be estimated at a quart; it was of precisely the same colour and consistence as that which issued from the perforating ulcers of the ileum. The soft and pulpy spleen weighed 8 ozs. The brain weighed 52 oz., and was excessively congested, internally as well as externally.

From a consideration of the symptoms, and *post-mortem* appearances of

the case, it seems most probable that the extravasation took place at different periods. The first, a limited one, on the tenth day; the subsequent and more profuse one on the fifteenth day. It is also noteworthy that the effect of the liberal exhibition of opium, although it relieved the pain of the first attack, may have predisposed to the second extravasation, by penning up the secretions of the bowels in the intestine above the seat of the ulcerations, and so increasing the pressure upon the wall of the bowel at the points weakened by loss of substance. The lower portion of the ulcerated bowel was closely contracted, while above it the intestine was distended by a great accumulation of liquid fæces. The perforating ulcers were about eight inches apart from one another.—*December 13, 1873.*

*Aneurism of the Abdominal Aorta.*—MR. TUFNELL brought under the notice of the Society a case of aneurism of the abdominal aorta, which had been systematically cured by position. It was (Mr. Tufnell believed) the first instance shown in that Society where the consolidation of the contents of the sac had been so induced, and the aneurism thereby cured. The patient was a Liverpool shipwright, aged thirty-one years. Some time since, after a heavy day's work, he experienced dorsal pain, for which he sought medical advice, and consulted a physician in Liverpool, who recommended rest and morphia injection. From this treatment he got much relief, and resumed his employment, but only to have a recurrence of the pain. The disease was then diagnosed as an abdominal aneurism, and he was advised to come to Dublin to be subjected to this treatment, by continued rest, under Mr. Tufnell's care. He arrived at the City of Dublin Hospital on the 7th of February last in a very excited and, at the same time, exhausted condition, the vessel in which he embarked having been wrecked on the voyage, and he and some of the crew having been rescued by a life-boat; but, being landed on a beach, wet through, and exposed to the gale for a whole night, the consequent suffering was very great. His pulse, upon admission, was no less than 170, and his nervous system quite upset. After a few days rest this excitement subsided. The aneurism was now carefully examined. It was situated two inches below the ensiform cartilage, and measured three-and-a-half inches in either diameter. A loud bruit and a soufflet accompanied the first sound of the heart when lying, but, as usual, was lost upon the patient's standing upright. The pulse at this time was 120 sitting, and 104 lying down, whilst upon moving across the ward it became excessively rapid. Treatment by position and a restricted but nutritious diet was commenced upon the 17th of February, and in thirty seven days the contents of the sac were solid. Subsequently symptoms of Bright's disease of the kidney showed themselves, which took the usual course, terminating life on the 7th of September, death being preceded by convulsions and coma, the urine at this time being

literally a mass of albumen. Upon examination of the body after death, the brain, heart, and lungs, were found healthy; serous effusion, to a certain extent, only existing in the right pleural cavity; but the liver was in a state of fatty degeneration. The aorta and large blood-vessels were free from atheromatous deposit. The aneurism sprang from the front of the aorta, immediately below the giving off of the cœliac axis, and three inches above the bifurcation. The sac of the aneurism was perfect, being traceable superiorly and inferiorly into the coats of the aorta. The contents were solid, disposed in concentric layers of fibrine, laminated, from the circumference to the centre. Posteriorly, upon removal from the spine, the column was found perfect, and the back of the aorta and the front of the spine as healthy as in any ordinary case, the smallest lumbar vessels being patent and discharging their normal functions. Through the aorta existed (posteriorly to the aneurismal tumour) a full and sufficient trajet for the blood to the parts below. The kidneys were both congested, but most dissimilar in appearance. The right was rather smaller than natural, whilst the left was greatly enlarged, measuring five inches in length by three and a-half inches across. The infundibulum was dilated, and upon section the organ exhibited a well-marked specimen of interstitial nephritis, with fatty degeneration of the epithelium. The renal vessels were perfectly normal on both sides.—*December 13, 1873.*

*Laceration of Lungs and Heart.*—DR. WHARTON wished to engage the attention of the Society for a very brief period while he gave an account of an accident which occurred within the last week, and exhibited the results of the accident, as shown in the specimens now presented.

The subject of the accident, a man about fifty years of age, was brought on an outside car to the Meath Hospital from the scene of its occurrence, a distance of less than five minutes walk. On arrival at the hospital life was extinct, and the thoracic walls highly emphysematous, the face pale, pupils dilated. It would appear he had been driving a float, and in some way mismanaged his horse, so that he was thrown off his seat, and crushed between the vehicle of which he was in charge and another which was passing in an opposite direction.

On extrication from his position it was observed that the man breathed. This, however, must be considered doubtful, if not impossible—most likely death was instantaneous. From examination of the specimens produced, it was seen that both clavicles were dislocated at their sternal extremities posteriorly; the upper six ribs on the left side were completely separated from their sternal attachments; the sternum was fractured transversely at a site corresponding to the space between the 6th and 7th ribs at their junction with that bone. On raising the ribs at the left side the thorax was found filled with blood, the apex of the left lung

anteriorly, and the posterior surface of the inferior lobe were torn. The pericardium was rent longitudinally and transversely, the edges of the rents being widely separated. In addition to such severe injuries, the right ventricle of the heart was ruptured to such an extent as to admit of the easy ingress of the right index finger.—*December 13, 1873.*

*Enteric Fever: Peritonitis without Perforation of the Intestine.*—*DR. NIXON* exhibited the intestines of a boy, aged nineteen, who had been under his care in the Mater Misericordiæ Hospital in September last. He had been ill for eleven days, suffering from slight diarrhœa and anorexia. On admission, his pulse was 136, soft and dicrotous; his tongue thickly coated and dry, but not fissured; the abdomen was greatly distended; there was extreme tenderness over the right ileo-cæcal region and *gargouillement*. The temperature was 102°·5F. There were no spots. The boy had the typical aspect of one suffering from enteric fever—the bright eye and the hectic flush of the cheeks. Diarrhœa was urgent and of the usual ochreous character.

On the following day the expression of the face entirely changed. The eyes were sunken; the flush of the cheeks had disappeared; he seemed dull and drowsy; temperature fell to 101°·5F.; the diarrhœa had lessened, but the abdominal inflation and tympanites had increased. The pulse was 108 and still soft. The urgent danger of the case seemed to be perforation of the intestines from great flatulent distension. Suitable measures were adopted to relieve symptoms, but without effect. Shortly after I left hospital, the patient vomited large quantities of brownish-black fluid; he became collapsed; the emesis continued incessantly until his death, which took place about three o'clock in the evening.

The *post-mortem* examination was made eight hours after death. The rigor mortis was well marked. On opening the abdomen, the parietal peritoneum was found reddened and injected, especially towards the right iliac region. The intestines were matted together by soft yellowish lymph, of a gelatinous consistence. There was no trace of pus or purulent fluid in the peritoneal cavity. The termination of the ileum, for about four inches, was covered by an organized false membrane, from which the general peritonitis seemed to have spread. Opening the ileum the solitary glands were found enlarged, producing a “sap-like” condition of the mucous membrane. Peyer’s patches, tracing them from the commencement of the lower third of the intestines, presented varying appearances of disease. In some the minute glandules were visibly enlarged, so that the surface of the patch looked coarse and granular, corresponding to what are known as the *plaques molles*. In other patches minute depressions were found as if the infarcted glands had burst and discharged their contents. Towards the ileo-cæcal valve the cavity of the intestine was markedly constricted, so that it was with difficulty the



little finger could be passed down the gut. The mesentery was studded over with enlarged lymphatic glands, and one of these, the size of a wall-nut, by its presence, caused the constriction. The ileac surface of the ileo-cæcal valve was covered by a blackish diphtheritic slough, partially detached, whilst beneath it lay the softened muscular fibres of the intestine. The adjacent peritoneal surface was covered by layers of false membrane. There was not the slightest trace of perforation of the intestine. The spleen was enlarged and softened. The bases of both lungs were somewhat congested. There was no evidence of tubercle in any of the organs.

The case was interesting in a clinical aspect, from the latency of symptoms prior to admission into hospital, considering the gravity and extent of the local lesion. It, moreover, illustrated the point that some of the worst cases of enteric fever are unaccompanied by eruption.

The *occurrence* of peritonitis from simple extension of diseased action through the coats of the intestine, without perforation, was comparatively rare. Dr. Nixon observed that in this, and a similar case which came under his observation, the effusion from the inflamed peritoneal surface partook of the characters of organizable or plastic lymph, whilst in peritonitis following perforation pus or purulent fluid was generally found in large quantities.—*December 13, 1873.*

*Deformity of the Hand resulting from the Cicatrix of a Burn.*—MR. THORNLEY STOKER said the specimen which he now brought under the notice of the Society, was the remains of the hand and a portion of the forearm of a woman admitted under his care into the Richmond Hospital, on the previous Saturday.

She was a healthy-looking, sanguine person, married, and the mother of several children, and about thirty-three years of age. She was subject, it appeared, to epileptic fits, and during one of these fits, about a year previously, had fallen into the fire and received a terrible burn involving her right hand and forearm, which resulted in the shocking deformity shown.

The fingers, except the ring-finger, of which the first, and part of the second phalanges remained, had been completely burned off, and evidently at that time a great fungous mass of granulations had sprouted up all over the hand, and having eventually skinned over, caused the very peculiar appearance now seen. The hand, greatly thickened, and with a few projecting knobs representing the fingers, could only in form be likened to a huge misshapen potato, or an enormously over-grown Jerusalem artichoke. The skin covering the parts which had been burned, presented none of the usual dense cicatricial tissue which forms after burns and in no part was there any tendency to contraction; it was of a pink colour and very thin, and at one point, on the back of the wrist, a large ulcer existed.

It was manifest that the case was one for amputation, and on Wednesday last he had removed the hand and part of the forearm by a double flap operation, being obliged to go as high as the middle of the forearm, as he could not have got healthy skin lower down to cover the flaps.

He had used Professor Esmarch's plan in amputating, and had found the loss of blood extremely small, and the action of the elastic bandages and tourniquet, favourable in the extreme. He considered the case of much interest as showing such an extraordinary degree of deformity; so strange, indeed, was the appearance of the member, that it could not, if its connections were hidden, be recognized as the remains of a human hand; and there could be no doubt that the hypertrophy and distortion of the parts was due, as he had stated, to the abundant masses of granulations which sprung up, and finally skinned over.—*December 13, 1873.*

*Carcinoma of the Liver.*—DR. QUINLAN brought forward a case of diffused carcinoma of the liver, which, he said, was taken from the body of a woman, aged sixty-seven, who was admitted into St. Vincent's Hospital at the beginning of the present winter session. On admission she was moderately jaundiced—the jaundice being of a greenish hue—and on digital examination of the liver, through the parietes of the abdomen, a hard tubercle was felt, clearly indicating the nature of the case. The jaundice remained about the same for a while after admission, when it suddenly became intense, and of a yellow hue, accompanied by acute pain in the right hypochondrium, and all the other usual symptoms of the passage of a biliary calculus through the duct. The pain was alleviated by the subcutaneous injection of morphia, and after two days ceased as suddenly as it had supervened, with a corresponding decrease of the jaundice. New symptoms now appeared, the right foot and leg swelling and becoming œdematous, along with slight ascites. The œdema extended up the right thigh to Poupart's ligament, and was traced to a hardening of the lymphatics and glands of Scarpa's angle. The jaundice gradually became intense; the ascites slightly increased; the patient became weak and torpid, and expired on the morning of the 7th December, 1873. Throughout this whole case there had been a tendency to constipation, and the urine had been loaded with bile.

On making a *post-mortem* examination attention was first directed to the abdomen. The peritoneal cavity contained a quantity of yellow serum, which, under the spectroscope, gave the absorption bands of bile, and also responded to the nitric acid test, and to Pettenkofer's test. The liver was studded, both on the surface and in the interior, with carcinomatous tubercles, the one felt on admission being very conspicuous. Two of the larger ones presented a button-like depression in the centre. The gall bladder was full of hard substances, evidently calculi. [Dr. Quinlan here opened the gall bladder and removed a number of

calculi, facettèd, seventeen in number, and weighing altogether 113 grs]. The common gall duct was imbedded in a mass of malignant matter, which pressed slightly on the vena porta, and which, by pressing upon the gall duct, had evidently caused the chronic jaundice. At the same time the duct had evidently undergone great dilatation by the passage of previous calculi. Careful search was made in the stomach, pancreas, and spleen, and in the general periphery of the portal system, for any malignant disease which might have been the source of the hepatic cancer. The only source of metastasis discovered, however, was the indurated condition of the lymphatic system of the right thigh, already alluded to. This induration extended to the intestinal lymphatic glands, and was evidently malignant. A few of the pulmonary glands, at the bases of the lung, were similarly but more slightly affected. The kidneys were healthy. Microscopic examination of the hepatic tubercles showed a mass of spindle-shaped cells with nuclei and nucleoli. Dr. Quinlan, in conclusion, said that this case was an example of jaundice, produced concurrently in the same case by the operation of two distinct causes—each cause, however, supplying its own means of exact diagnosis at the time of occurrence; also of the connexion between cancer of the liver and biliary calculi.—*December 13, 1873.*

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

### CONTENTS.

THIRD SERIES, No. XXVIII.—APRIL 1, 1874.

#### PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. VIII.—Fractures of the Skull, with Special Reference to Operative Interference; with Cases. By ANTHONY H. CORLEY, M.D., F.R.C.S.I.; Surgeon to Jervis-street Hospital; Lecturer on Surgery, Carmichael School of Medicine, - - - - -	305

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. Typhoid Fever: its Nature, Mode of Spreading, and Prevention. By WILLIAM BUDD, M.D., F.R.S., - - - - -	311
2. Lectures on Syphilitic Osteitis and Periostitis. By JOHN HAMILTON, Surgeon to the Richmond Hospital, &c., - - - - -	315
3. Animal Physiology: the Structure and Functions of the Human Body. By JOHN CLELAND, M.D., F.R.S., &c., - - - - -	316
4. A Treatise on the Diseases of the Tongue. By W. FAIRLIE CLARKE, M.B. (Oxon.), F.R.C.S., - - - - -	317
5. Die latente Gonorrhœ im weiblichen Geschlecht. Von DR. EMIL NÆGGERATH, in New York, - - - - -	324
6. A Handbook of the Theory and Practice of Medicine. By FREDERICK T. ROBERTS, M.D., B. Sc., M.R.C.P., - - - - -	326

#### PART III.—MEDICAL MISCELLANY.

Transactions of the Medical Society of the College of Physicians:—	
Cases of Œsophagismus or Spasmodic Closure of the Œsophagus. By A. W. FOOT, M.D.; Junior Physician to the Meath Hospital,	327
Case of Dislocation of the Kidney; Renal Abscess; Recovery. By W. B. PEEBLES, M.B., Ch.M., Univ. Dubl.; L.K.Q.C.P., &c. -	338



## Proceedings of the Dublin Obstetrical Society:—

An Anomalous Labour. By CHARLES DRUMMOND MOUTRAY, L.R.C.S.I., &c., - - - - -	343
On the use of the Perchloride of Iron in Post-partum Hæmorrhage. By LOMBE ATTHILL, M.D., &c., - - - - -	346
Post-partum Hæmorrhage, treated by the Application of the Solid Perchloride of Iron to the Interior of the Uterus. By A. HILL RINGLAND, A.B., T.C.D., L.K.Q.C.P.I., &c., - - - - -	356

## Proceedings of the Pathological Society of Dublin:—

DR. KIDD on Colloid Carcinoma of Ovary, - - - - -	370
DR. GRIMSHAW on Cerebro-spinal Fever, - - - - -	375
PROFESSOR W. STOKES on Ulceration of the Cartilages of the Knee- joint, - - - - -	376
DR. A. W. FOOT on Extreme Amount of Intestinal Ulceration in Enteric Fever, with Constipation, - - - - -	377
DR. BENNETT on Stricture of the Urethra, - - - - -	378
DR. LOMBE ATTHILL on Malignant Tumour of the Uterus, Removed by the Ecraseur, - - - - -	381
DR. A. W. FOOT on Hepatic Carcinoma, - - - - -	382
DR. WALTER SMITH on Congenital Excess of Ureters, - - - - -	384

## Transactions of the Ulster Medical Society:—

DR. JOHN MOORE on Excision of the Elbow, - - - - -	385
DR. JOHN MOORE on Case of Labour Complicated with Abnormal Presentation, - - - - -	385
DR. JOHN MOORE on Amputation and use of Esmarch's Means of Restraining Hæmorrhage, - - - - -	387
DR. JOHN MOORE on Dislocation of Hip-joint Reduced by Flexion and Rotation. - - - - -	388
DR. FAGAN on Noma Pudendi, - - - - -	388
DR. H. S. PURDON on Lupus Erythematosus, - - - - -	388
DR. CHARLES on Gastric Ulcer, - - - - -	390
DR. WHITLA on Horse-shoe Kidney, - - - - -	391
DR. DAVID JOHNSTON on Post-partum Hæmorrhage, - - - - -	392
DR. H. S. PURDON on Delirium Tremens, - - - - -	394
DR. MURNEY on Disease of the Knee-joint, - - - - -	396
DR. RICHARD BARNETT on a Case of Fracture of the Inferior Maxillary Bone, - - - - -	397
DR. RICHARD BARNETT on Oblique Fracture of Lower Maxillary Bone, - - - - -	399

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

APRIL 1, 1874.

---

### PART I.

### ORIGINAL COMMUNICATIONS.

---

ART. VIII.—*Fractures of the Skull, with Special Reference to Operative Interference; with Cases.* By ANTHONY H. CORLEY, M.D., F.R.C.S.I.; Surgeon to Jervis-street Hospital; Lecturer on Surgery, Carmichael School of Medicine.

SOME very important cases of fractured skull having been recently under my care in Jervis-street Hospital, I am induced to publish them, with one of a more remote date, in the hope that their narration, with the observations they suggest, may throw some additional light on a subject of the greatest importance. I shall detail the cases as briefly as possible, and only dwell on the essential particulars.

CASE I.—J. G., a labouring man, aged about forty-five, was struck on the forehead by the corner of a brick. He was insensible for a few minutes only, and was admitted into the hospital, October 1st, 1867, with apparently a small cleanly-cut wound, about an inch and a half above the right supra-ciliary arch. The finger, inserted into the wound, detected easily a piece of bone, about the size of a shilling, fractured and depressed. It sloped in such a manner as to show that the lowest part was the most driven back, so much so that the whole thickness of the undepressed bone could be felt with the nail. As there were no symptoms present the edges of the wound were brought together, and in

twenty-six days the man left, completely recovered. For the first few days he got a little hydrarg. c. cretâ and James' powder; but, as no bad consequences from the accident supervened, this was omitted, and regulation of diet was the only additional interference.

CASE II.—Joseph M., aged three years, fell from a window into a paved area, a height of ten feet. The upper and right side of his head first touched the ground, and a sharp projection seemed to have struck the lower and back part of the parietal bone. When admitted he was in a state of insensibility, and, as well as could be judged, there was paralysis of the left side of the body. The cranial lesion was most remarkable. Two small scalp wounds were apparent, but only one led to the bone. Almost the entire parietal bone was depressed, so much as to give that portion of his head the appearance of perfect flatness, as compared with the opposite side. A more irregular depression could be found posteriorly and inferiorly, with a superficial wound, and at this place a distinct pulsation was visible. The question of operation immediately suggested itself, and I sent for my senior colleague, Dr. Stapleton, who was also on duty. I confess I did not contemplate with much hopeful feeling the operation which would be necessary—namely, the elevating of the whole parietal bone, with extensive exposure, &c., and was therefore much pleased when signs of returning sensibility became manifest. When Dr. Stapleton saw him he was almost perfectly conscious, and we agreed that, under the circumstances, careful watching, regulation of diet, and the exhibition of small doses of grey powder, would be the proper procedure to adopt, and we were rewarded by the immediate result. The symptoms disappeared, the paralysis rapidly got better, and the appetite became good. Twice matter formed under the scalp, and it had to be incised. One of these incisions was towards the posterior depression, and as it remained open exploration with a probe was performed. A piece of dead bone was detected, which seemed separate from the large depression, although not completely detached. The sequel of this case is a melancholy one, although it does not detract from its value as a clinical illustration. As I did not consider the child quite safe until the piece of bone alluded to should be separated, I detained him in hospital nearly four months. He improved in every way, picked up flesh, and never had a bad symptom. Unfortunately a malignant type of scarlatina

accidentally found its way into the hospital; my patient was attacked, and succumbed after a week's illness.

CASE III.—James M., aged thirteen, was admitted October 10th, 1873, suffering from fracture of both bones of right forearm, and depressed fracture (simple) of right parietal bone, from a blow of a windlass handle. The symptoms usually attributed to "concussion" of the brain were present—viz., almost complete insensibility, cold surface, feeble pulse, and quiet respiration. The special point of interest about this case was that, after reaction was fully established, the insensibility recurred, but again passed away without any special treatment. The depression, although a simple one, was manifest and unmistakable. On the 6th of December the patient left perfectly recovered.

CASE IV.—Tommy M., aged four, admitted August 25th, 1873. A few minutes before his admission he had fallen from one flight of stairs over the balustrade down to another, and had alighted on his head. He was perfectly insensible, and was breathing stertorously, and on examination a deep depression was easily found at the posterior part of the right parietal bone. It was also discovered that his right femur was fractured about the middle third. As the symptoms persisted for some hours, the surgeon on duty called for a consultation, with the view of considering the propriety of operative interference. By the time, however, the staff of the hospital had assembled, there were some signs of amendment, and further consideration of the case was postponed. From this date the favourable progress continued, and the child was discharged cured October 25th.

CASE V.—R. G., aged fourteen, was admitted December 21st, 1873. He had fallen through the trap-door of a loft on his head, and sustained a compound depressed fracture of limited size at the posterior superior part of the left parietal bone. He was scarcely more than stunned by the fall, and recovered his consciousness almost immediately. So little did he seem to have been affected that he could with difficulty be induced to remain in the hospital. A small piece of the injured bone became necrosed; but, as no bad symptoms were at any time present, he refused to stay in hospital, and now occasionally attends as an out-patient until such time as the lamina separates, pending which I cannot pronounce him *absolutely* out of danger.



The foregoing cases were, for most of the time, under my own immediate observation and in my charge; but for two of them I am indebted to my colleague, Dr. Meldon, who, knowing the interest I take in injuries of the head, kindly transferred them to my care, although they were admitted during his month of duty. I felt it scarcely necessary to detail the treatment, which may be summarized thus:—Constant watchfulness of symptoms, especially the pulse and temperature, purgatives on the supervention of reaction, and, in some of the cases, small doses of grey powder, occasionally combined with James' powder. My object in detailing them is to illustrate and assist in proving the statement made by Mr. Jonathan Hutchinson, in one of the ablest lectures which even he has ever delivered, on "Compression of the Brain."<sup>a</sup> He there states his belief "that depression of bone is rarely the cause of the symptoms of compression." A full recognition of the truth of this opinion is of the utmost importance to the practical surgeon; for if we examine the record of cases in which operative interference has been resorted to in injuries of the head, we find that in by far the greater number the object was to raise depressed bone. I do not wish in the following observations to inculcate absolute avoidance of all operative procedure in depressed fracture; for in that case I should have little justification for the several times I have been myself obliged to resort to the trephine or elevator, but I wish to show how much the brain will bear from depressed bone with impunity, and how little, therefore, can be expected from its elevation. If the operation itself were one altogether devoid of danger, it would be an ungracious task to endeavour to shake the young surgeon's confidence in its efficacy; but unfortunately a calm consideration of many of the cases of recovery from cranial injuries, in which the patients were operated on, must convince us that they recovered in spite of, rather than because of, the interference. Exactly a century ago a distinguished fellow-citizen of ours<sup>b</sup> published a series of cases of injury of the head, in which the almost invariable treatment—following the advice of Le Dran, Pott, and other leaders of surgical opinion—was trepanning, not only for depressed fracture, simple or compound, for effusion of blood, for formation of matter, but even for simple fissure—"to anticipate inflammation and its consequences!"

It is refreshing to read now how Mr. Dease describes a case of

<sup>a</sup> London Hospital Reports. 1867. P. 13.

<sup>b</sup> Dease on Injuries of the Head, 1778.

fracture of the skull without any symptoms of cerebral injury, and tells how he trepanned in three places, and that, notwithstanding, the patient afterwards had "irregular shiverings," signs of meningitis, and died. Even at that time he was beginning to see that the operation did not always increase the patient's chance of recovery, and his later cases are treated somewhat in accordance with this change in opinion. But to Sir A. Cooper and Abraham Colles belongs the chief merit of limiting the list of cases in which the use of the trephine is justifiable; Colles, in particular, by showing the danger of the operation. It is impossible to forget the anecdote which he relates<sup>a</sup> of a certain Dr. Evatt, who conceived the idea that lunacy depended on the skull being too small for the brain, and actually prevailed on the superintendent of an asylum to let him experiment on his patients. Three were selected and trephined, and two died! Now, here was the operation performed under the most favourable circumstances. No immediately antecedent injury of brain, or other condition threatening life, and yet two out of three died, and, therefore, died of the operation alone. When it is considered that trephining may be fairly described as the gradual, slow, and scientific production of a compound fracture of the cranium, it should be sufficient to make us understand thoroughly and appreciate fully the conditions imperatively demanding its performance. I am, in laying down these conditions, confining myself solely to the injury of bone, as to dwell on all the others would be beyond the limits of this paper. Sir A. Cooper states that in every case of compound depressed fracture the trephine should be used, whether symptoms be present or not. Cases I. and II. prove sufficiently that such directions require much modification. I shall enumerate the injuries of the cranial bones in which trephining *has been* employed, and then try to define in what cases it was necessary.

1. *Simple fissure.* — For this fracture the operation should never be performed. True, that accompanying the injury there *might* be localized extravasation of blood; or, subsequent to and consequent on it, there might be formation of matter, which may require the application of the trephine, but the operation then has no reference to the fracture.

2. *Simple comminuted fracture.* — A fracture may be simple externally, but the inner table may be more extensively fractured, and fragments may wound the dura mater or brain. This condition

<sup>a</sup> Lectures. Edited by M'Coy.

cannot be guessed at until symptoms of intra-cranial mischief arise—for them, and not for the fracture we may trephine.

3. *Depressed fracture*.—I do not make any distinction between simple depressed and compound fracture as to operative treatment. The latter is more liable to be followed by intra-cranial mischief. As long as no symptoms are present, or *if present, until we have tried all other means of removing them*, we should not operate. If obliged to interfere we do so with little hope, as the symptoms are most likely to own an origin other than the depressed bone.

4. *Depressed fracture, comminuted*—including that which is known as “punctured” fracture, such as may be produced by the stab of a pointed weapon, kick of horse, or blow of a sharp stone.—In many cases of this description it may be necessary to operate at once, *whether symptoms be present or not*. If the surgeon has reason to believe that in a punctured fracture spiculæ of bone are impinging on the surface of the brain and lacerating it, he is bound to interfere at once. The cause, nature, and position of the injury, and the peculiarities of the symptoms, if any be present, will be all of value in assisting him to arrive at a correct determination. However, in this case—the only one in which I would sanction interference without symptoms—much must be left to the experience and judgment of the surgeon. With reference to this particular point, I shall draw attention to the remarkable cases reported by my colleagues, Drs. J. Stannus Hughes<sup>a</sup> and Meldon,<sup>b</sup> and it will be confessed that they more than justify the latter part of Liston’s aphorism—“that scarcely any injury of the head is too grave to be despaired of.”

<sup>a</sup> Irish Hospital Gazette, No. 193.

<sup>b</sup> Dublin Medical Journal, No. 4, 1872.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

*Typhoid Fever: its Nature, Mode of Spreading, and Prevention.*

By WILLIAM BUDD, M.D., F.R.S. London: Longmans, Green, and Co. 1873.

THIS work is welcome as the result of the mature thought and large experience of one who may be considered as a pioneer in the study and prevention of enteric fever. The author informs us that the substance of the work before us is to be found in the various papers upon typhoid fever, which he published, from his first, in the *Lancet*, in 1857, down to those in the *British Medical Journal* of 1860. His first chapter is written under the following heading from Bishop Wilkins:—"Time wears off the fictions of opinion, and doth, by degrees, discover and unmask the fallacy of ungrounded persuasion." We wish this quotation were applicable only to the study of enteric fever, instead of, as it is, to nearly all studies in medicine.

Dr. Budd estimates that an average of 15,000 persons die, and 140,000 persons are attacked, annually in England by enteric fever, and gives a graphic description of the horrors of epidemic diseases—a description too easily guessed at by many of our readers, and more thoroughly realized by those who remember 1846 and '47 in Ireland, than even Dr. Budd himself can conceive. The relation between disease and pauperism is so well known in Ireland that a proof of it from an English source is a twice-told tale.

Dr. Budd tells us that from returns made in 1838 by the medical officers of twenty unions and parishes in the metropolis, 13,972 cases of claims to relief, on the grounds of destitution, were created during that year by attacks of fever alone, and that in 1,281 cases the attacks proved fatal.

Dr. Budd contends strongly for the belief in the contagious nature of enteric fever, and quotes with surprise the statements of the authors of such works as Tanner's "Practice of Medicine," and Hooper's "Vade-Mecum," remarking "that this conclusion should be



persisted in is, I repeat, very perplexing." We quite agree with Dr. Budd, and believe that there can be no doubt whatever of the contagious nature of enteric fever, and unfortunately we have met with instances where there was but little question as to its arising from this source. The number of facts in favour of contagion are so numerous that we scarcely can see how any one can doubt the evidence. Dr. Budd quotes an epidemic of fever in the village of North Tawton, in Devonshire, as a proof of the spread of typhoid by contagion. After describing the extremely bad sanitary condition of this village—which, apparently, is equal to any of the worst "mere Irish villages" which our English friends speak and write of with so much horror—Dr. Budd makes a remark, which, together with a good many similar ones, we think, he should have thought twice of before giving forth with the weight of his authority, namely—"these conditions existed for many years without leading to any of the results which it is the *fashion* to ascribe to them." [The italicising of "fashion" is ours.] This is just the sort of sentence for an ignorant and obstructive poor law guardian to make capital out of, when required, against his will, to enforce sanitary law. Dr. Budd writes of the bad sanitary condition of North Tawton "as powerless to *generate* fever" without a more specific element—contagion. This is followed by other illustrations to the same effect.

The author considers "that all the emanations from the sick are, in a certain degree, infectious." But "that which is cast off from the intestine is incomparably more virulent than any" he seems to imply further on by the statement—"they (contagious effects) never occur except where no sufficient provisions have been made for preventing the discharge from the human intestine from contaminating the soil and air of the inhabited area;" that it is *only* by means of the intestinal secretions that the disease can be carried.

"Like malignant cholera, dysentery, yellow fever, and others that might be named, this is one of the great group of diseases *which infect the ground*. Hence the quasi-miasmatic character attaching to them all, which has misled so many observers as to their true mode of spreading."

We think Dr. Budd really wastes much time and space trying to prove that enteric fever *is* contagious, as most experienced observers admit it to be so. But evidently Dr. Budd's argument is that if the disease is contagious it is impossible that it has a pythogenic origin, or any other origin than contagion. There is really little or no connexion between the questions; and we are sorry

to find Dr. Budd going in for what may be called the contagionist theory against any other theory of causation for this disease. Every disease must have had an origin. Each may, no doubt, have been created as a distinct entity by the Almighty Framer of all things, but the variety and modification of diseases are so great, and the results of modern investigation so encouraging, that we are forced to believe in, at all events, the *possibility* of the spontaneous origin of disease. In trying to prove his case of contagion, and *only* contagion, as a source of enteric fever, our author is carried away by his enthusiasm; for instance, he writes:—"To inhale sewer emanations in a great city is, therefore, under conditions of the most frequent occurrence, *actually to inhale the very quintessence, so to speak, of a pre-existing fever.*" This, whatever it may be worth, is a fact of the most real kind."

We are afraid that such sweeping assertions, with its italics, are calculated rather to retard than to promote sanitary reform.

After quoting several instances of the origin of enteric fever from polluted water and from a polluted stream, in which latter the disease was probably introduced from a distance and spread down the stream, Dr. Budd remarks:—"The only inference it seems possible to draw from these facts is, that while sewage charged with the specific fever poison is all potent in breeding fever, sewage not so charged has no power to breed it at all."

Dr. Budd corroborates the views held by Murchison, Pettenkofer, and others, that the contagious principle of the enteric fever discharges is developed by changes which set in after those discharges have been exposed to external influences, but at the same time admits that fresh discharges, with ordinary care, are "not very dangerous to the inmates of the sick room." He seems to think that the general diffusion of the poison may be explained without any such hypothesis—merely by the minute division and dispersion of what might previously be considered as masses of contagious matter from the patient's bowels. We do not think, however, that the diffusion is so very great as Dr. Budd seems to imply by the phrase that "to inhale for an instant the exhalations of a sewer, or to walk down a lane or alley, &c., &c., is often to be stricken with this fever unto death." If the poison were really so generally diffused and so subtle, we can scarcely account for the escape of so many persons who are constantly exposed as above. We know of an association, of which we ourselves are members, which performs the duties of inspecting the fever dens of Dublin,

we think, up to the present, without any serious consequences. How can the immunity of these gentlemen be consistent with the general diffusion of such a virulent contagion?

Dr. Budd gives several notable instances of supposed communication of enteric fever by clothing through third persons. These examples certainly are remarkable, and contrary to the general impression with regard to the communicability of the disease. He also refers to contaminated water and milk as means of propagation of contagion, and gives instances to prove the spread of contagion through contaminated air—in other words, by dust or miasm. From what we have stated above our readers will be aware that Dr. Budd aims at proving that contagion is the sole cause of enteric fever. He further on in his work makes the following statement in which he puts his case fully and concisely before his readers:—"Typhoid fever is not only self-propagating, but arises in no other way." As we have already stated, there can be no question about the self-propagation of the fever, and Dr. Budd's work is a most valuable contribution to the already numerous pieces of evidence of the contagious nature of enteric fever. At this point we must disagree with Dr. Budd, for he has not in any way disproved the pythogenic origin of enteric fever by proving its contagiousness. Dr. Budd seems to imply that small-pox, scarlatina, and measles, with which he classes typhoid, are universally admitted to originate only by contagion. Now, Dr. Budd, we are certain, is aware both of the researches of Dr. Salisbury, as to the cause of measles, and Dr. Carpenter, of Croydon, as to the association of scarlatina with certain special conditions. Both these observers have *substantially* raised the question as to the spontaneous, or, as we may say pythogenic origin of these forms of disease, and no one has yet substantially disproved their conclusions. That contagion may be made, by stretching arguments to their utmost, to account for the origin of all cases of contagious fever, is evident; but, nevertheless, there are strong arguments in favour of enteric fever being caused by decomposing faecal matter, typhus by over-crowding, &c., which have not yet been overturned. That certain diseases which seem to be contagious may have an independent origin, is pretty well shown by experiments on the production of *septicaemia*, in which many decomposing animal matters, from different sources, have produced forms of disease indistinguishable from pyæmic poisoning by pus from specific suppurations. The remark of Trousseau, that enteric fever "shows a great proneness to return to the same houses," is

used by Dr. Budd as an argument in favour of contagion and contagion only; whereas, if we remember rightly, we have heard the same remark often quoted in favour of pythogenesis, and that only as a cause of the disorder. So much for the way premises may be employed to produce conclusions.

While Dr. Budd's arguments in favour of contagion are perfectly convincing, and cannot be refuted by any arguments we are aware of, and while his suggestions for the prevention of contagion are admirable, we must repeat that his arguments against pythogenesis and spontaneous generation are the weakest possible, and altogether of a negative character.

---

*Lectures on Syphilitic Osteitis and Periostitis.* By J. HAMILTON, Surgeon to the Richmond Hospital and to Swift's Hospital for Lunatics; late President of the Pathological Society of Dublin. London: J. & A. Churchill. 1874. 8vo., pp. 108.

SOME of the happiest hits in diagnosis—and, as a consequence, some of the most successful cures—which we have ever known, consisted in tracing a puzzling symptom or group of symptoms to a syphilitic contamination of the system, and every man engaged in general practice should make himself familiar with the manifestations of that persistent and Protean taint. In these lectures, Mr. Hamilton, who has for years had an extensive experience in venereal diseases, presents us with an account of the injuries it works in the various bony structures. According to him it affects the osseous system under three forms:—periostitis, the bony node, and the soft node. He adduces some cases to show how syphilitic periostitis of the rib may be mistaken for pleurisy, and syphilitic inflammation of the periosteum over the great trochanter may produce symptoms which resemble sciatica. But still more interesting are his cases of periostitis of the cervical vertebræ, causing pharyngeal tumour, and various paralytic phenomena, and a very striking narrative of a syphilitic periostitis of the orbit, which several eminent surgeons mistook for malignant disease. Epilepsy he has seen in several cases produced by syphilitic disease of the skull, usually aggravated in its consequences by intemperance, but almost invariably curable. He makes, however, the important observation that, in some syphilitic cases (fortunately they are extremely rare), the seizures depend, not on



disease of the cranial bones, but on gummata of the dura mater, and in these the prognosis is much less hopeful. The soft node, according to the author, is usually a late manifestation, occurring in persons of the strumous diathesis. He dissents altogether from the doctrine that these often terrible affections are in any way due to the administration of mercury. In early cases he trusts for a cure to a three months' course of that mineral; but, in late cases, he usually prefers the iodide of potassium. Locally, he applies blisters; and, in very obstinate cases, divides the periosteum and keeps the wound for some time open. In the soft node he gives the iodide, applies tincture of iodine, and specially warns us against opening it, a procedure which induces the death of the bone beneath.

---

*Animal Physiology: the Structure and Functions of the Human Body.* By JOHN CLELAND, M.D., F.R.S.; Professor of Anatomy and Physiology in the Queen's College, Galway. With 150 Engravings. London and Glasgow: W. Collins, Son & Co. 1874. Pp. 325.

THIS little work is one which we commend very strongly to the notice of teachers of physiology. The chief object of the writer is to supply to readers, not very familiar with anatomical details, as complete an account as possible of the functions of the body. He has also aimed at furnishing the junior student of medicine with a compendium of physiology, which may assist him in obtaining a clear idea of the principles of the science, and prepare him for the perusal of works of a more elaborate character.

That there is a real want of a good elementary treatise on physiology, adapted to the requirements of the medical student during his earlier years of study, every teacher of physiology will admit. In fact many students are deterred from commencing the study of physiology during their first year by the cumbrous nature of some of the so-called text-books. To be able to recommend to one's pupils a book of moderate dimensions, is, in itself, an advantage—an advantage which is much enhanced when the book is found to be also accurate in its matter, clear, concise, and well illustrated.

Professor Cleland escapes from a fault not uncommon in small hand-books intended for the use of students in medicine. He avoids the dogmatic style as much as appears to be possible in such a

treatise. His little work is at once popular and philosophical. He aims rather at teaching his pupils to know facts, and reflect upon them, than to know them merely in order to answer certain questions at an examination.

The illustrations are numerous and well executed, many of them are original, and of those which are not the authorship is always duly acknowledged. A feature of the work, worthy of remark, is a glossary at the end, giving brief explanations of the technical physiological terms used throughout the book, with their derivations. This glossary, will, it is hoped, make it easier for the student to get some hold of a kind of information of which he is at present lamentably ignorant.

Of course there are not a few points to which severely critical physiologists may take exception, each from his own point of view. Taken as a whole, however, and fairly estimated at what it is intended to be, this book may be pronounced an admirable one. The author, in our judgment, wisely avoids touching upon some of the later topics of physiological research.

Tolerably well established truths are sufficient to give the beginner food for reflection, and in physiology there are enough of these not only to exercise the memory but to charm and fascinate the mind with the science of all others best calculated to develop the mental qualities most needed by him who aims at becoming an observant and accomplished practitioner.

---

*A Treatise on the Diseases of the Tongue.* By W. FAIRLIE CLARKE, M.B. (Oxon.), F.R.C.S. Renshaw, London. 1873. Pp. 244.

IN his Preface Mr. Clarke remarks:—

“Notwithstanding the importance of the tongue in the human economy, it has not received the amount of minute study which it deserves. Its diseases have not formed the subject of any special treatise in this, or, as far as I have been able to ascertain, in any other country; and the works which have dealt with its semeiology have furnished us with nothing but the most general conclusions.”

This is, no doubt, the fact, and constitutes an adequate reason for the publication of the present essay; and even if the handsome volume which the author has submitted to the profession be not

entirely exhaustive, or systematic, in its treatment of the subject, it certainly supplies the desideratum he refers to in a great degree with signal success, and carries the reader over a very wide range of literary research and practical detail.

This treatise opens with a long introductory chapter upon the anatomy and physiology of the tongue, which, however accurate in its details, might, we think, be judiciously omitted without doing damage to the value of the work, as it contains no special facts beyond those which can be obtained from the more legitimate sources of information supplied by any of our numerous anatomical and physiological text-books. This observation applies, indeed, to the general practice in fashion of ushering in every monograph upon the diseases of any special organ by an essay upon the ordinary anatomical and physiological arrangements concerned; and for the practice (which savours somewhat of mere "book-making") we have perhaps this fashion, and not the author, to blame.

Next follows a chapter upon examination of the tongue, which, we think, the author has left unnecessarily meagre. Amongst other things, we miss any allusion whatever to any system of examining the mobility of the tongue—a point of considerable importance in reference to the diagnosis of various paralytic affections, or any instructions as to the methods of testing variations of its tactile and gustatory sensibility—a subject which would well deserve some elucidation under this heading, presenting, as it does, frequent clinical difficulties in examinations of this organ.

The more special part of the work includes chapters upon congenital defects, diseases of nutrition (atrophy and hypertrophy), parasitic diseases, inflammatory affections, syphilitic affections, cancerous affections, tumours, and injuries of the tongue. In reference to the way in which these several topics are handled by the author, we can speak in terms of almost unqualified praise. In discussing them Mr. Clarke's treatise gives evidence of a considerable amount of careful and extensive literary research, acute powers of personal observation, and much facility in accurate and pleasing writing.

In reference to the subject of congenital defects of the tongue, we have some interesting illustrations of the fact of the wonderfully slight effects produced upon the faculty of articulation by a considerable loss of substance of the tongue. Mr. Clarke details the classical case of M. de Jussieu (1718), of a girl born without any trace of this organ, and who, at the age of fifteen, when examined, spoke

intelligibly and fluently. It was only remarked, that in pronouncing some of the consonants (viz., c, f, g, l, r, s, t, x, and z) she had some little difficulty. He again reverts to this subject (in a future chapter) in an entertaining account of the practice prevalent during the middle ages, and even still so in certain Eastern countries, of cutting out the tongue, as a mode of punishment for certain high political or religious offences—a mutilation after which powers of intelligible, if not perfect, speech remained in almost all instances. This, too, agrees with the observations of surgeons upon cases of operative ablation, or of sloughing of the tongue. While on this matter—of the dependence of accurate articulation upon the perfection of the mechanism of speech—we think it right to lay some stress upon the fact (the converse of the above) illustrated by Mr. Clarke in reference to the importance of early operation for the removal of congenital malformations of the tongue, however slight—viz., that a defective habit of articulating once acquired, as the result of such a condition, will rarely be relieved by a late operation, no matter how perfect we may leave the articulating organs. All practical surgeons are well acquainted with the truth of this remark, as it applies to late operations performed upon a cleft palate. In the case of congenital defects, likely to interfere with the child's learning the proper formation of words, of the tongue as of the palate, it suggests the urgent necessity of operation before habits of faulty enunciation have become confirmed.

Atrophy of the tongue may be looked at from various aspects. It may be unilateral or bilateral; it may occur as a solitary and primary lesion, or as a part of a more general atrophy of the muscular system (as in *progressive muscular atrophy*, or in the *glossolabio-laryngeal paralysis* of Duchenne, or [rarely] in ordinary *hemiplegia*). The nerve which essentially presides over the nutrition of the tongue appears to be demonstrated to be the hypoglossus; and the determination of the real nature and source of lingual atrophy presents in individual cases some of the most difficult and beautiful problems in the diagnosis and localization of nervous lesions. Mr. Clarke, accordingly, classifies these cases into two groups, according as the lesion of this nerve is cerebral or extra-cerebral—a grouping of them which is plainly of great importance in a diagnostic and prognostic point of view. The diagnosis must be sought for by an appeal to the considerations of the questions—whether the atrophy affects one or both sides? whether evidences of simultaneous lesion



of the eighth pair are present? and by the discovery of some external local sign, tumour, &c. Amongst numerous illustrative cases of the kind given by the writer, one of syphilitic origin, in which permanent amendment followed a mercurial course, is worth remembering; as also one from the practice of Sir J. Paget, in which rapid recovery took place after the removal of some pieces of dead bone from the region of the base of the skull.

The chapter upon that remarkable, and almost universally congenital, disease, hypertrophy of the tongue (macro-glossia), is one of the most valuable in Mr. Clarke's work. He gives an important list of forty-three cases in which treatment was adopted, collected from different sources. The list presents us with an unexpectedly favourable view of operative measures as applied to this horrible affection, as in only three out of the whole number was surgical interference unsuccessful. The surgical measures have consisted variously of compression; compression with astringent applications (so strongly advocated by Lassus); amputation by the knife; removal by ligature, either alone or with amputation; the use of the ecraseur. Ligature of the lingual arteries was unsuccessful in the only case in which it is recorded (Liston). Referring to this table, Mr. Clarke makes the following remarks:—

“From a consideration of this table, it appears that the disease is most common in the female sex; that, if it be not congenital, it almost always commences during the earliest years of life; and that, when the hypertrophy is only slight, great benefit may be expected from regular compression, combined with the use of emollient or astringent lotions.”

Cases which have not been congenital are such as have followed an epileptic fit, or a fever, or some local disease, as an abscess or ranula, or seemed to be due to salivation.

As to the vegetable parasites (*oidium albicans*, *leptothrix buccalis*) which may occur upon the tongue, we have the following observations:—

“As long as persons are in good health they need have no fears as to the development of these fungoid growths. The normal state of the saliva is unfavourable to them, and it is only when the secretions become vitiated and acrid that they begin to develop.”

Animal parasites (such as the guinea-worm and the *trichina*) are alluded to, as having been met with in the tongue, as pathological curiosities.

On the important subject of glossitis, Mr. Clarke adopts an anatomical basis of classification, dividing cases into three groups, according to the depth of tissue involved. The first class embraces cases in which we have to do with a proliferation and subsequent morbid changes of various kinds of the lingual epithelium, and includes, in addition to all the numerous varieties of "fur," the so-called *psoriasis linguæ* and *ichthyosis linguæ*. Of these affections, Mr. Clarke gives instructive examples, though he, with reason we think, doubts the propriety of applying such terms to them. He believes these affections in their real characters to have a near relation to epithelioma.

The second class includes the remarkable "vesicular disease" of the tongue, first described by Sir James Earle; and the more important, because more common, affection, *chronic superficial glossitis*, so named originally by Copland.

As to the etiology of the affections included within the limits of these two groups, Mr. Clarke expresses a strong conviction as to the existence of a syphilitic taint in the great majority of instances. While fully admitting the preponderance of this element as a cause of the affections in question, we, at the same time, think that he has scarcely given enough of weight to, or devoted an adequate amount of discussion to, their other possible sources—viz, local irritation (for example, from the very frequent source of the excessive use of the cigar or pipe), or gastric and dyspeptic derangements, several cases illustrative of which are given. More particularly in reference to *chronic superficial glossitis*, some observations which would assist in supplying the reader with definite views as to any means of endeavouring to arrive at a diagnosis of its syphilitic or non-syphilitic nature, would be of great practical value.

The third group includes those cases of inflammation of the deeper structures and substance of the tongue, to which the term "glossitis" is more commonly restricted. Numerous cases, belonging to this heading, arising under very various circumstances, are admirably detailed by Mr. Clarke, and illustrate the very different causes to which acute glossitis may owe its occurrence. Mr. Clarke draws a conclusion, to which we feel, however, bound to demur, as it involves an important principle of practice, and may be somewhat misleading if unexplained. He says:—"From the foregoing cases we may conclude that acute glossitis, however formidable in appearance, has generally a tendency to spontaneous recovery." If this statement be taken to mean that cases of acute glossitis are

rarely fatal, even when left to themselves, it may or may not be true; but if by it the writer means to imply that as a rule such cases should be treated on simply expectant principles (and his subsequent explicit statement of treatment seems to imply as much, p. 137), assuredly he did not derive this suggestion from a study of the details of the series of cases he has himself related; as, on a review of them, we find that in all of the instances given (with one exception) the most active surgical measures were employed—such as general blood-letting (in some cases to a large amount), frequent and repeated leechings, deep incisions adopted in the majority of cases, and general anti-phlogistic treatment of an active kind. To our minds, on the contrary, if any logical conclusion be drawn from this special list, that conclusion would be that which seems to have been already endorsed by almost universal surgical authority—viz., that there are few diseases in which active surgical interference can be so signally beneficial. Mr. Clarke enumerates, and illustrates by cases, several of the events which may supervene upon acute glossitis, some of them sufficiently remarkable—viz., abscess, ulceration, sloughing, adhesions, necrosis of the hyoid bone, and others. One of these from its urgency, the risks entailed by a mistaken diagnosis, and its ready treatment when recognized, seems to us to be more particularly worthy of a passing note, that is, the formation of an abscess far back in the base of the tongue, which, from its situation, presses upon and obstructs the glottis, and may thus induce urgent symptoms of dyspnoea, and may even simulate laryngeal disease. We have ourselves witnessed more than one case of this kind, in which the patient was saved from the operation of opening the air passages externally, if not from death, by the recognition of the real situation of an abscess of this kind, which a guarded incision of the base of the tongue reached, with the result of producing instant relief.

A long chapter is deservedly devoted to syphilis, as it attacks the tongue. This prominence is acknowledged to be merited, and is fully justified by the following remarks:—"Of the various diseases which affect the tongue, syphilis is the most common. Thus I find that five-ninths of the cases of tongue disease I have noted were of this nature." The varieties of syphilitic diseases of the tongue are numerous. Primary chancre has been observed. Fournier has noted this situation in six out of 824 collected cases of chancre. Amongst secondary affections, we may have mucous patches, superficial ulcerations, or that peculiar and distinctive condition to

which reference has already been made under the title *Chronic Superficial Glossitis*, of which the writer gives a good description, which should be consulted by those who desire a more accurate knowledge of this important and imperfectly described disease. The discussion of the affection constitutes perhaps the most original part of Mr. Clarke's treatise. Its main features are—a swollen condition of the whole tongue; general furring, with isolated red smooth patches sometimes slightly swollen, and with denudation of the epithelium; superficial irregular ulceration of exquisite sensitiveness. There is little doubt of the comparative frequency of the occurrence of such a state of tongue, and equally little of its frequent association with syphilis, of which it appears to be a symptom which may spread over a very extended period of the stages of that malady. We have already expressed our belief that Mr. Clarke has scarcely given enough of prominence, however, to the agency of other causes to which he makes allusion—viz., gastric or dyspeptic derangements, local irritation, or the prolonged use of medicaments, more particularly of anti-syphilitic medicines (mercury and iodide of potassium). Of the tertiary effects, we have here an adequate discussion of the gummatous tumour, which, affecting as it mainly does, the tissues of the central fibrous raphe, has usually a median situation, with its various events of absorption and cicatricial contraction producing deep and indelible fissures, or of calcification and the production of remarkable lingual calculi (of which some curious pathological instances are given), or of its becoming disintegrated and producing the typical indurated syphilitic ulcer of the tongue, the diagnosis of which from cancerous ulcer is fully gone into.

The chapter upon cancer of the tongue illustrates most specialities of malignant disease as it attacks this organ—the occurrence of epithelioma as the prevalent type; rareness of its occurrence as a secondary disease; its greater frequency in men than in women; its unusually rapid and fatal progress, &c. Some of Mr. Clarke's results are obtained from an examination of thirty-nine cases of his own, of which he has been able to trace the histories. One of his most important and practical conclusions is the framing of a verdict from them in favour of operation, the average term of life in the cases operated upon being more than twice that of those left to the natural course of the disease—"The average duration of life was, in those cases which were not operated on, forty-two weeks; while, in those which were, the average was eighty-six weeks;" a result which tallies well with the conclusions arrived at by Paget. As to the



various methods of removal, the author speaks highly of the galvano-cautery, and of the ecraseur. He says:—"If an ecraseur is selected which is fitted with rather a coarse cord composed of several strands of wire, and if it be worked very slowly—say one half-turn every minute—it will generally be found that there is no hæmorrhage whatever." He fairly objects to the method by ligature—the inconvenience, disagreeableness, and danger of the presence in the mouth for several days of a large sloughing and putrefying mass.

Too much praise cannot be given to Mr. Clarke for the careful and laborious selection of cases, illustrative of his various subjects, with which his work is so abundantly adorned; and in reference, notably, to the matter of tumours of the tongue, he has collected together a series of remarkable and unusual cases of much interest and value, which show what exceptional and peculiar manifestations of disease this organ may be subject to.

In his last chapter, upon injuries of the tongue, he lays stress upon the wonderful powers of union and repair possessed by this organ, giving cases in which parts of it, almost completely severed, have been preserved by careful adaptation and suturing.

We believe Mr. Clarke's treatise to be a valuable addition to our surgical literature, and that it will form a most useful work of reference for those anxious for information upon the subject it treats of.

*Die latente Gonorrhö im weiblichen Geschlecht.* Von DR. EMIL NÆGGERATH, in New York. Bonn: Max, Cohen, & Sohn. Pp 125.

UNDER the title "Latent Gonorrhœa in the Female," the author describes certain forms of uterine disease, which he attributes to infection from latent—*i.e.*, apparently cured—gonorrhœa in the male. To describe these forms, and to point out their etiology and treatment, is the object of the treatise.

The author, as a gynæcologist, does not consider it to be his place to investigate the latent affection in the male; but, from the facts which have been brought under his notice in his own immediate sphere of practice, he has come to the conclusion that fully 90 per cent. of those believed to be cured are, in reality, not so, but retain, often to their lives' end, the elements of infection.

The phenomena produced in the female by this infection are, according to the author, those of perimetritis. They vary, however, not only in intensity, but also in form, and may be arranged under four heads, viz.:—1°. Acute perimetritis; 2°. Recurring perimetritis; 3°. Chronic perimetritis; 4°. Ovariitis. With each of these forms is associated a catarrh of some part of the mucous membrane of the genital organs, while in some few cases the catarrh is the only symptom present. As to the importance of a fungus universally met with in this secretion, the author declines to give any definite opinion, but invites the attention of mykologists to the subject.

However definite this fourfold division may be, and however important as to prognosis, the various forms are found to pass one into the other, and it has often happened that all four have been observed in the same patient in the course of months, or even of years. Each form is then discussed by the author in detail, and the notes of fifty cases, including *post-mortems*, are recorded.

The effect produced by latent gonorrhœa on the fertility of the woman is, according to the author, most serious. Out of 81 cases of marriage where the husband had previously suffered from gonorrhœa, 31 women became pregnant. Of these, 5 aborted, 8 had premature labours, and the remaining 23 were confined at the full time.

A further analysis of the history of these 23 women shows that—

12	women	had	but	1	child.
7	„	„		2	children (1 a case of twins).
3	„	„		3	„
1	„	„		4	„

In addition to which there were 5 abortions.

One fourth of the treatise is devoted to the consideration of treatment, and the author concludes by recording some instances in which—1°. The latent form produced an ordinary gonorrhœa; 2°. After a long period of latency a gonorrhœa spontaneously appeared; and, 3°. Where a man became affected with a gonorrhœa from infection given by him to his wife, through the latent form, many months previously.

The author, convinced as he is, that gonorrhœa—a disease generally looked upon as transitory in its nature, and confined in its effects, to the person affected with it (if only the most obvious

caution be observed), is, in reality, most permanent in its infecting influence, and the cause of intense and life-long suffering, of sterility, and even of death itself to the virtuous woman whose husband has years before suffered from it—invites the most careful consideration and investigation of the subject from those who can in any way throw light upon it.

---

*A Handbook of the Theory and Practice of Medicine.* By FREDERICK T. ROBERTS, M.D., B.Sc., M.R.C.P., Fellow of University College, Assistant-Physician and Assistant-Teacher of Clinical Medicine at University College Hospital, &c. London: H. K. Lewis. 1873. 8vo., pp. 1,043.

WE postponed noticing Dr. Roberts' Handbook until we had time carefully to examine it. We have done this, and we feel bound to speak of it in the highest terms. In our opinion the author has most successfully executed the very difficult task he undertook—it is not too much to say that in the work will be found a concise and accurate epitome of the information contained in our best monographs. Before describing the individual diseases of the several organs or systems an outline has been given of the clinical phenomena which indicate a morbid condition of each and of any modes of physical examination employed in their investigation. This we consider one of the most valuable features in the Handbook, as the information contained in the chapters on the physical examination of the chest, the general diagnosis of affections of the lungs and pleuræ, diseases of the circulatory organs, diseases of the abdomen, and diseases of the nervous system, are admirably fitted to encourage and guide the student in honest clinical work. We are persuaded the present edition will be rapidly exhausted, and we hope the second one will contain a few woodcuts of those subjects of which no author can give an accurate idea in words. It is a pity so admirable a work should want illustrations of the microscopic objects found in urine.

## PART III.

### MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

#### TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

*Wednesday, March 11th, 1874.*

JAMES FOULIS DUNCAN, M.D., President, in the Chair.

*Cases of Œsophagismus, or Spasmodic Closure of the Œsophagus.* By  
A. W. FOOT, M.D., Junior Physician to the Meath Hospital.

THE following four cases, which I have the honour to read before the Medical Society of the College of Physicians, appear to me to be examples of a not very common affection, to which Vogel,<sup>a</sup> or, according to others, Mondière,<sup>b</sup> many years ago gave the name of œsophagismus. The affection is a spasm, or spasmodic contraction of the œsophageal muscles of such a kind as to interfere with deglutition. Three of the cases relate to men, and one was in a female, and are given from my own notes taken at the time. It is proper to observe, that in none of them was there any evidence of organic disease within or without the œsophagus; that none of them were cases of what Sir Henry Marsh described as the regurgitating disease, inasmuch as there was no evidence in any of them of the food having entered the stomach; and, further, that I do not use the word hysteria in reference to any of them, although certain forms of dysphagia are observed in the condition of the system which goes by that name.

The first case was that of a pale, timid-looking lad, of sixteen years of age, from the county Tipperary, who was admitted into the Meath Hospital on account of a difficulty in swallowing food, liquid or solid. He was of a spare, nervous habit, weighing eight and a half stones; his height was five feet three and a half inches. He described his complaint thus:—"When I take a bit it stops in my chest

<sup>a</sup> Dict. de Med. et Chirurgie Pratiques, tome xii., p. 145. 1834.

<sup>b</sup> Nouv. Dict. de Med. et de Chirurgie, tome xii., p. 96. 1870.



until I take a sup, or it comes back;" the "sup" generally pushed it on, but, when it failed to do so, the whole returned into the mouth. He had been subject to this ailment for six months; it had not increased in severity during the last two months; he could assign no cause for it but "cold." He had not got thin since it happened, but, on the contrary, had grown stouter; his appetite was good, and he had the wish to eat, but was frequently baffled in his attempts to swallow. He referred the stoppage to a point about two inches below the notch of the sternum; the cause of the stoppage he spoke of as a "lump," which rose to this point from some part further down; and he said that sometimes he could get this lump down by drawing a full breath. Before actual stoppage of the food had occurred, its passage through the œsophagus used to be attended with rattling and roaring noises. He could not swallow any liquid if cold; cold milk or water could not pass, but warm milk or tea could; he had great difficulty in swallowing any "sticky" substance, such as stirabout. He could not eat bread and butter; the first thing that regularly stuck in his throat was a piece of bread and butter—even bread and milk was often stopped; he subsisted principally on warm milk and tea. The difficulty of swallowing was greater at some than at other times; he found it always worse when he was hungry. When the stoppage occurred, if the bread was not pushed on by a mouthful of warm milk or tea, and he tried to send it on by forcible voluntary efforts, he very seldom succeeded in doing so—on the contrary, his eyes filled with tears, and the food was pushed back into his mouth. He found that it was of no use to try to force it on into the stomach; it was better to let it wait a while, as it often then went on of its own accord. The "bit" has remained in the œsophagus as long as an hour before it got into the stomach. This resistance to the passage of the food was unattended with pain, though accompanied with many disagreeable sensations. When the food was returned it was mixed with much mucus; it never had any acid taste; he felt sure it had never passed into the stomach; he used to know quite well when it did so, and upon such occasions used to give a sigh of relief and satisfaction at the conclusion of his struggle. The food was sometimes returned with such force as to come through the nose. He had no symptom of derangement of the stomach, neither heartburn, flatulence, nausea, or palpitations; he never vomited; he quite understood that the food always stopped short of the entrance of the stomach, and its return was never attended with discharge of the contents of the stomach. He had no cough, stridor, or alteration of voice; nothing could be found abnormal about the heart and lungs. At one time I thought he might have had enlarged bronchial glands, but the intermitting nature of the dysphagia was against this supposition. There was no evidence that the œsophagus had ever been in any way injured. In every respect he felt in good health; there was no spinal irritation.

He was very subject to hiccough—he used to observe that it was very constant on him. He derived great benefit from bromide of potassium, in doses of thirty grains three times a day; I had previously given him iodide of potassium, with hemlock juice and tincture of belladonna, and had blistered the back of his neck without producing any obvious improvement. On the third day after commencing the bromide of potassium he was able to drink *cold* milk, which he had not done for months, and continued daily, with much pleasure, to test his improvement with cold drinks, which hitherto had been special provokers of the dysphagia. On one occasion, when getting better, he found himself eating some dry bread without thinking of it, and as soon as he recollected what he was doing the difficulty of swallowing came on. I tried on one occasion to pass a large flexible catheter along the œsophagus, but even in the mouth and pharynx the muscular agitation and resistance were so great that I thought it useless to continue the attempt. He went home after having been eighteen days in hospital; he considered himself cured. Six weeks afterwards he wrote to me—“The bit stops but seldom, but it never stops bad; I don’t mind it at all now.” He was the son of a gamekeeper, and had led a healthy and active life in the country, taking plenty of exercise, and being very much in the open air.

The second case was that of a man aged twenty-four, from the county Leitrim, who was admitted into the Meath Hospital complaining of an incapacity of swallowing food, liquid or solid, but especially solid. Pure liquids, when taken alone, would generally pass into the stomach, if taken in small quantities and swallowed in a cautious manner; but if solid or dry food were taken before or with the liquid, he felt that the mass was stopped short of the stomach by what he described as a lump rising, which, after a struggle on his part, used to succeed in ejecting the food from his mouth, but without the gush or violent action of true abdominal vomiting. He was frequently made eat under observation that his account might be verified. Bread and butter, for example, would be masticated, and the buccal and pharyngeal actions subservient to deglutition properly performed, but before the piece of bread was consumed he would give notice that the “lump” was rising, his eyes then filled with tears, his voice assumed the husky and choking sound of a person who had a large piece of food in the œsophagus, and the mass would be returned and ejected with much mucus. The sensation of “a lump rising” was sometimes felt when there was no food being swallowed. On one occasion, immediately after one of these returnings of food, a probang was passed into the œsophagus, no obstruction was felt, nor did the instrument excite pain or spasm. The dysphagia was, to some degree, intermitting; at times he could drink freely without regurgitation, and milk was, in consequence, his chief diet, both before and after his admission to hospital. Apples, plums, and other acid fruits he found to be the

most difficult things to swallow ; meat or broth he had a great dislike to ; the difficulty of swallowing was greatest in the morning. He was not emaciated, but had a healthy appearance, and was sunburnt from out-door labour. He had been affected with this complaint, with short intermissions, for three years ; it commenced with hiccough, and he knew no cause for it. He presented, on admission, various dyspeptic symptoms ; he had a dull pain in the stomach, relieved by food, and occasional pyrosis, eructations, and palpitation of the heart. The urine contained a small quantity of albumen, probably attributable to the state of his digestion. The skin of the whole body, but especially over the abdomen, was remarkably dry and harsh. This patient took valerian and camphor for some time, without improvement, but derived considerable benefit from a mixture of subnitrate of bismuth and hydrocyanic acid. After using these medicines he became able to overcome the tendency to regurgitation, to prevent the return of food, except about once a week, when the "pushing upwards" became too strong for him. While thus apparently improving, he became affected with enteric fever, which ran its course without any unusual events, except that it entirely modified for the time the condition of the œsophagus. During the second week of the fever he felt so completely free from the spasm that a plum was given him as a test, since this kind of fruit was more liable than anything else to excite the spasm. The plum passed readily into the stomach, but it is to be feared that he ate more than the experimental one, because, though no disturbance of the œsophagus was excited thereby, yet he got such a smart attack of diarrhœa that a repetition of the test was forbidden. While the fever continued, food was not returned, although occasionally the "lump" was felt rising, but on convalescence the former symptoms reappeared, with heartburn every night, and as he was anxious to go home on his recovery from the fever, he left hospital before any further efforts for his improvement could be made and in much the same condition as when admitted. This man had never sustained any injury of the œsophagus, and had no signs or symptoms of inter-thoracic disease.

The third case was that of a stout, healthy-looking man of intemperate habits, thirty-two years of age, from the county Roscommon, who was admitted into the Meath Hospital, complaining that he could not swallow solid food, and sometimes even liquids ; he had suffered from this state of things for three years. Shortly before it came on, he got a squeeze round the neck from a drunken man ; he said his complaint commenced one night, as he was coming home, in a stiffness in his jaws, and that since then he had not eaten "a pound of meat." The dysphagia was not worse when he came under observation than it had been at first ; he had no alteration of voice ; he spoke of a squeezing sensation about the neck, on a level with the thyroid cartilage, there was no enlargement of the thyroid gland ; he had palpitations, pain, and lightness across the forehead ;

he never vomited, but could not get his food to pass into the stomach. Physical examination of the neck or chest threw no light on his condition. The first time an attempt was made to pass an instrument down the œsophagus he was so much alarmed and offered so much nervous resistance that it could not be done; on another occasion an instrument was passed a short distance and met with much spasmodic opposition. The induced current was passed between the back of the pharynx and the lower dorsal vertebræ on several occasions. After a six weeks' stay in hospital, he went home unimproved.

The remaining case was that of a woman, aged forty-three, married, and mother of six sons, who was admitted into the Meath Hospital, with total inability to swallow. When I first saw her she told me that not a drop of water had passed her throat since that day week; she did not look emaciated, and had a fresh colour; said that she had no pain anywhere, and that she was in perfect health but for the difficulty of swallowing. Her pulse was strong and full; there was no evidence of inter-thoracic disease. She had a bilious tinge in the eyes, but there was no enlargement or tenderness of the liver. She had great thirst, which she relieved by holding water in her mouth; some of this occasionally would slip down her throat, but she could not take a drink of water. She had been in another hospital, where she had been fed by the rectum with brandy and beef-tea. The history she gave me was that, six months before she came under my notice, while under the influence of intense grief, she had a vomiting of blood, the blood came up, a mouthful at each time on three occasions in the same day—4 a.m., noon, and 3 p.m.—without effort. She did not feel anything give way, but felt a kind of grip under her heart. A week before this event she had vomited a teaspoonful of blood. She had not menstruated for the last thirteen years—*i.e.*, not since she was thirty years of age; she had never injured her œsophagus in any way. For twelve days after the hæmatemesis she could not swallow anything, then for a period of six weeks she could swallow better, but was never well able to swallow meat; during one week only was she able to swallow bread and butter; the remainder of the time she lived on arrowroot, milk, and beef-tea. She spoke of the cause of the dysphagia as a “constriction,” and when asked to indicate its situation referred it to various places from the thyroid gland, which was not enlarged, downwards. This sense of constriction came and went; when it came a feeling of thirst came with it, and went away when the spasm passed off. She spoke of her throat then as being “open.” Her voice was not affected; she could speak and sing well. Sour things particularly excited the spasm. The age of this woman and the history of hæmatemesis made it necessary to consider the diagnosis carefully. I regarded it to be a case of a spasmodic nature, and not due to aneurism or structural disease, on account of the variability, the sudden violence or completeness of the



dysphagia, the absence of emaciation, and various minor belongings of the case which are more easy to apprehend than to describe. As she told me, the first day I saw her, that not a drop of water had passed her throat since that day week, I ordered her to be fed by the rectum, to have a suppository with a grain of extract of belladonna, and a belladonna plaister to the back of the neck; but two hours afterwards, before any remedies had been employed, she had swallowed a mug of milk and two of beef-tea, and that without difficulty. The constriction returned the same day; she could not get bits of ice down, but held them in her mouth, watching for the remission of the spasm, since, as soon as any water had slipped down, she was then able to swallow. In a few days her throat remained "open" for several hours, but the spasm would return, and the throat remain "closed" for twelve or sixteen hours at a time. As she asked for something to "clean her stomach," I ordered her dilute muriatic acid in infusion of quassia, and from this medicine she said she found great benefit; she generally could swallow it easily, although ordinary acid drinks used immediately to excite the constriction. Her improvement was marked by her asking for arrowroot, by an increase in strength, and by her leaving her bed, which she had kept from weakness. Then she attained to buttered toast, which she had not been able to manage for the last six months. She left the hospital in fifteen days, saying she was quite well. She several times got belladonna by the mouth, but it always appeared to make her mouth drier, and to excite the spasm. She considered that the muriatic acid and quassia had cured her, and continued to take it as an out patient. There was no return of the spasm, and a month after she left she was able to undertake an arduous situation as private nurse in a case of ovariectomy, and performed her duties in a very efficient manner.

In the two first of the above cases there was no cause assigned for the occurrence of the complaint, in the third it was attributed to a squeeze of the throat, in the fourth to intense grief. The whole four were persons of the disposition called "nervous;" that is, their nervous system was easily impressed, their fears or emotions readily excited; but I had no reason to believe that their symptoms were either feigned or exaggerated, or in any way under their control. It is well known that in emotional conditions the functions of the œsophagus are very liable to perversion, the difficulty with which an angry child swallows the cold water prescribed for its temper is an illustration. In these cases the dysphagia was certainly œsophageal, and not pharyngeal, and in such cases it is foolish to speak of them as hysterical, in the sense of their being at all under the control of the patient, or within the influence of their will. Pharyngeal deglutition, which is only involuntary from our inattention to it, may be affected by the will, but not œsophageal, which is a reflex action; the successive excitement of different points of the mucous

membrane of the œsophagus, by the food passing from above downwards, provokes a reflex contraction of the involuntary muscles of the tube; in the normal condition this contraction only occurs in limited segments, corresponding to the points which receive the sensory excitement; when the sensibility of the mucous membrane of the tube is exaggerated the result may be a spasm, which will oppose an obstacle to the passage of food. The sensibility of the mucous membrane of the tube is not within the cognizance of common sensation; it is unfelt, and under the influence of the pneumogastric nerve, and so this over excitability, which is the starting point of the reflex muscular contraction, is not painful, though it may be attended with sensations which cause alarm and apprehension of suffocation. The diagnosis of this affection (œsophagismus) is based upon the suddenness of its occurrence, the variability of its intensity with various kinds of food, its intermittence, the co-existence of other symptoms, especially hiccough; the absence of other causes of dysphagia—mechanical, inflammatory, or paralytic. The œsophageal vomiting in these cases is manifestly different from gastric vomiting, in the absence of nausea, and of contraction of the muscles of the stomach or abdomen, nor has the returned food any sour or acid taste.

Dr. Graves<sup>a</sup> mentions, under the title of a “Curious Affection of the Organs of Deglutition,” two cases of this kind of spasmodic and intermitting dysphagia, both in men. One was a nervous young clergyman, who complained of various symptoms indicating debility and dyspepsia, but was chiefly annoyed by a painful and convulsive struggle, as he expressed it, which sometimes took place between the bit he had swallowed just before it entered the stomach and a something which seemed to resist its further passage downwards. This lasted only for a few seconds, and was very distressing both to himself and the spectators, for, of course, it usually occurred at meals, and rendered him unwilling to dine in society. In the other case these sudden attacks of temporary dysphagia became so habitual, that the gentleman never ventured to eat, unless a glass of water was within his reach, for in him the stopping of the descent of the bolus of food was attended with an urgent sense of suffocation. This gentleman, an excellent anatomist, thought that the sense of suffocation was entirely nervous, or, at least, had nothing to do with any mechanical obstruction in the glottis arising from the neighbourhood of the descending food. Dr. Graves observes that in both of these cases the cause of the disease appeared to lie in the increased, or rather deranged, sensibility of the œsophagus itself.

Sir Henry Marsh’s<sup>b</sup> observations are “on a peculiar morbid affection of the stomach, characterized by regurgitation of its contents without

<sup>a</sup> *Dubl. Med. Jour.*, iii., 167.

<sup>b</sup> *Dubl. Med. Jour.* xxiii., 437, and *Dubl. Quart. Jour. Med. Sci.* xvi., 481.

nausea." All the curious cases in this paper are examples of disgorging food, of regurgitation, or rejection on the part of the stomach of a meal which had been swallowed without impediment; all those which I have read were cases in which the difficulty was to get any food into the stomach. The cases of regurgitation appear to be more common in young females; those of œsophagismus in young males.

There was nothing very remarkable in the treatment of these cases. Two of them left the hospital in much the same state as when admitted; one of the others got well while taking dilute muriatic acid and quassia, to which she attributed her improvement; the remaining one, who was almost cured, derived speedy and marked benefit from the use of bromide of potassium.

The PRESIDENT said he remembered a case of spasm of the pharynx that occurred in his father thirty-six or thirty-eight years ago. Dr. Hutton and Mr. Richard Carmichael attended him, and for a week he was unable to swallow a drop of water or a morsel of food. He had no fever. He was not aware that there was any particular treatment, but he remembered distinctly Dr. Hutton asking if he was subject to gout. He was thinking of bleeding, but it was not done, and after a week—not of agony, for he suffered no pain but of hunger, for he could not eat, though desirous of doing so—suddenly the power of deglutition returned. He might possibly have been leeches, but no other treatment was adopted. For a week he had the idea of death from starvation before him, and he need not say the sense of relief was very great when he was able to eat again. He was then about fifty years of age, and though he lived thirty years after, he never had a return of the attack.

DR. ATTHILL said, as far as his experience went, these spasmodic attacks did not prove fatal, but they were very interesting, specially as regards the difficulty of swallowing certain kinds of food. He had at present under his care a boy twelve years of age, remarkable for the precocity of his intellect. He was at the head of his class in school, and was particularly good in mathematics. He was fairly circumstanced as regards air and exercise. He enjoyed a meat diet, but could not swallow it. He could eat anything that did not require mastication—bread, but not the crust, puddings, milk, &c.; but the moment he was given animal food the power of swallowing was gone. He would sometimes pass an hour in trying to masticate and swallow a square inch of meat. In his case there probably was the same enlargement of the bronchial glands, because the cervical glands were a little enlarged. The treatment adopted was the exhibition of the syrup of the iodide of iron. He had put the boy on a diet composed of milk, eggs, &c., and had ordered him to take plenty of exercise in the open air.

DR. M'SWINEY had had some experience of three cases of œsophagismus, and was bound to say that the picture drawn by the gentleman who had read the paper closely corresponded with the clinical features of the disease, as observed by him. He had had three such cases in his practice. They occurred in men of from eighteen to thirty years of age, and all three recovered. One of them in particular he watched with great interest for a very long time, and its principal characters were as follows:—First of all, the morbid state was preceded, and then accompanied, by a most enormous secretion of mucus—that was a very remarkable feature in the case; a great quantity of thick glairy mucus was being almost constantly ejected by the patient, more particularly when an attempt to eat was made. In the next place, it was noticed that there must have been occasionally a very tight constriction in the lower part of the œsophagus, because sometimes a piece of meat that was attempted to be swallowed one day would cause impermeable closure of the œsophagus for twenty-four or thirty hours, and at the end of that time would be rejected, and recognized as the same piece of meat that had been previously introduced. The spasm had then relaxed of course. At the same time he observed that, accompanying this great tightness of the œsophagus at one part, there must have been an enormous dilatation of the tube higher up, because it was often the case, that a very large quantity of liquid or solid food would be retained in the œsophagus, packed up like a pudding, as it were; none of this went into the stomach, but it would ultimately be rejected by the œsophageal vomiting, so well described by Dr. Foot. Another observation made was, that warm liquids could alone be swallowed. Cold liquids produced intense spasm and stricture, that was never overcome so as to allow them to pass, and the cold liquid was invariably returned. A probang, or an œsophageal tube, or bougie, passed into the stomach produced great relief, and removal of the stricture for some time, and that was an observation made by Sir Philip Crampton, who used to relate the results of his great experience in these nervous affections. In one case, he used to tell, he was always applied to by a lady, whenever she was going to dine out, to have a probang passed, in order that she might be able to dine with comfort, which this proceeding always enabled her to do. This lady had suffered from the affection for years. There was reason to believe that the condition of the mucous membrane, which secreted the mucus, was one of passive sanguineous congestion, because in one of these cases blood was sometimes brought up in small quantities, about a teaspoonful at a time, and this would be continued for twenty-four hours, perhaps, at intervals, and followed by a cessation for months, from which circumstance he was led to believe that there was a chronic congestion of the mucous membrane. These cases, observed by him (Dr. M'Swiney), were of the class of diathesis and temperament which Dr. Foot called the



nervous temperament. Further, no medicine seemed to exert any permanent effect, except some of those drugs known as "anti-spasmodic." Hydrocyanic acid, uncombined with any other remedy, and taken, in appropriate doses, three or four times a day, in water, Dr. M'Swiney had found give most relief to his patients. He (Dr. M'Swiney) wished to add that, in his experience, the state of the weather exercised considerable effect on the subjects of this affection. Damp, sultry, heavy weather always increased the distress; whilst in dry, bracing, cold weather the patient was always better. In the case most carefully observed by him very distressing dyspeptic symptoms were occasionally present, and gastric and œsophageal pain, from flatulent distension, was often felt. All three finally got quite well, as before mentioned.

DR. H. KENNEDY said:—The subject brought so well before us is one of much interest, and cases of this kind may be found scattered through the periodicals. In considering it, I would ask attention to the facts which occur in other mucous canals; as, for instance, the urethra in both male and female. Here, as you all know, the irregular action of the muscles leads to stoppage in the flow of the urine, and I have no doubt the same thing exactly occurs in the intestinal tube. I have seen cases, even in fevers, where there was no loss of the senses, but in which the obstruction for the time proved very serious. These considerations have led me to the conclusion, that when temporary obstruction occurs in the œsophagus, it is due to muscular spasm, most probably situated at the cardiac end of the tube; and this would appear to be borne out by what Dr. Foot stated, that in some of his cases warm fluids could be swallowed when cold ones could not. Under this idea, it is possible a warm bath might act favourably. Besides spasm, however, it has seemed to me that a certain amount of irritation, possibly inflammation, exists in some of these cases. In one case which I saw, besides the difficulty of swallowing, the patient complained of a sense of soreness; and this led me to order a medicine which may be worthy of notice here, and which certainly proved very successful in that case—I mean the nitrate of silver, which was given in the fluid form, with the idea that so it would destroy the over-sensibility of the mucous membrane. And so it proved, or seemed to prove. Formerly these cases were treated mainly by opium, used so as to dissolve slowly in the mouth, and also applied externally. My experience of them is the same as that of the author, for, whilst some have proved very obstinate, in no instance have I known the affection prove fatal.

DR. WALTER SMITH mentioned a characteristic case, which he had had an opportunity of observing for the last seven years. In November, 1866, a gentleman, aged twenty-nine, was attacked with cholera of a

grave form. He passed safely through it, but during his convalescence well-marked œsophagismus was developed. For a period of ten weeks he was unable to introduce the slightest particle of food, solid or fluid, into his stomach. Whatever he swallowed passed into the œsophagus, but near its termination it seemed to be suddenly arrested by some obstruction, and not an atom passed into the stomach. A few minutes after deglutition a portion was returned, and in a short time the whole was regurgitated from the œsophagus. At that juncture he suffered from soreness of the throat, and an acute pain across the chest when he tried to swallow fluids. At the same time hiccough and a copious secretion of saliva took place, which continued for several minutes, and he used to eject as much as four large cupfuls during the day. The attacks continued in an intermittent fashion for ten weeks, and on one occasion the spasm lasted for 68 hours, and matters assumed a more serious aspect until the spasm suddenly disappeared after the application of a large blister over the epigastrium. Since that time he had had repeated opportunities of observing the recurrence of the spasms. The worst that occurred during the last few years was in last December, when he was suddenly seized with the spasm while eating. This attack lasted 100 hours, and during the whole of that time not an iota of food passed into the stomach. On two occasions Dr. Smith demonstrated that it was an œsophagismal spasm. A short time after swallowing milk a firm, conical clot, or curd of milk came up, evidently a cast of the œsophagus, being just its shape and diameter, and which presented no evidence of having been acted on by the gastric juice. Like the cases narrated by Dr. Foot, there could be no doubt these spasms were purely œsophageal, and not pharyngeal, or gastric. There was not the slightest trace of hysteria, or of any organic disease, or of lesion of any kind. The affection also showed its capricious relation to treatment, for it might be said the spasms began when they liked, and ended when they chose, without any clear submission to treatment. Once they subsided after a blister, and on another occasion the hypodermic injection of the twentieth of a grain of apomorphia produced its nauseating effect in five minutes. The pulse ran up to 104, green bilious vomiting took place, the scanty contents of the stomach were evacuated, but the spasm returned immediately afterwards. During 1871, '72, and '73 the gentleman had frequent recurring attacks of these spasms. The duration was usually a few hours, but sometimes longer. As in the case referred to by Dr. Atthill, the œsophagus seemed to show a special objection to meat. He had no warning of the attack.

DR. FOOT made a few observations in reply. He was glad to hear of the treatment adopted by Sir Philip Crampton, that of passing a probang, which was new to him. Dr. Kennedy's remarks reminded him of a bad case, that subsided when the individual was put into a warm bath, which

went to support his view, that warm fluids should always be given to the patient.

*Case of Dislocation of the Kidney ; Renal Abscess ; Recovery.*

By W. B. PEEBLES, M.B., Ch.M., Univ., Dub. ; L.K.Q.C.P., &c.

MR. PRESIDENT AND GENTLEMEN,—Dislocation of the kidney is a subject more suited to a Surgical Society, but as the following case had a medical aspect when it came under my care, I have ventured to bring it forward here, in the hope that it may not be uninteresting.

On the 29th of February, 1872, a young married lady, recently recovered from small-pox, had a phaeton turned over upon her by a runaway horse some miles from town. She was found underneath it, on her right side, insensible. It was resting upon her. There was no mark of violence, save a cut on the chin. She was removed to a gentleman's house in the vicinity, and seen by several physicians. The gentleman under whose care she was placed writes to me:—"There was complete insensibility, as well as collapse, of all the powers of life. So much so, that it seemed very doubtful for some hours as to whether she would ever rally from this condition, and when she did, reaction set in very slowly. There was at first a tendency to retention of urine demanding the use of the catheter, but this only once. Having satisfied myself that no bones were broken, and that there was no paralysis, I set myself to try and discover if there could be rupture of any internal organ, but found there was not. As to concussion, I believe there was concussion—not, perhaps, in the common acceptance of the term as regards the great centres of the human system, but of almost every organ in the body. My attention, after a little, became specially directed to the lumbar region, more especially to one side. I rather think I leached it, and remember distinctly painting it over with strong tincture of iodine, and then, preparatory to her removal into town, strapping, padding, and bandaging."

There is no mention of any examination of the urine, nor of the almost incessant vomiting which occurred.

Towards the end of March the patient had so far recovered that she was moved into town in the horizontal position, and next day seemed so well that she was urged to try to walk. She exerted herself to walk into the next room, and dined heartily. The consequence was, that I was sent for to see her two days after for symptoms of acute nephritis on the left side. She had pain shooting upwards from the site of the kidney into the chest, downwards towards the thigh, and forwards to the front of the abdomen. There was constant micturition, nausea, vomiting, and fever. The urine was scanty and high-coloured.

After two days, when the violence of the symptoms had abated, and she could turn in bed, she felt what she called a "gizzard" loose in the

abdomen, following the movements of the body. The part of the abdomen towards which it might gravitate at the time became painful. When it was felt near the stomach food passed in seemed to strike against it. When the upright position was assumed it fell downwards. It was tossed about by the movements of flatus in the bowels. The agony attending all these movements was so great that a satisfactory manual examination was out of the question; but I felt convinced that it could only be the kidney held by an extempore mesentery. Dr. Law, who soon after saw the case, was of the same opinion.

It seemed probable that the edge of the vehicle, resting directly on the left side of the loins, had contused the deep parts against the spine, while a good deal of clothing and the cushion had saved the skin. That the kidney was then loosened in its bed, and that it dropped out afterwards, when the upright position had been assumed.

Measures were adopted, with the view to insure adhesion as near the kidney's bed as possible, but the surroundings of the case were not favourable. This lady had formerly been under my care for local and constitutional symptoms of phthisis. After a lengthened treatment they all disappeared, except a very rapid pulse. Many of them now returned, and for a month periods of comparative ease alternated with attacks of nausea, diarrhœa, perspiration, coughing, and irritable bladder, with discharge of reddish clots of lymph. Spasms in the psoas muscle took place. When the limb was tied down there was no annoyance, but if left to itself the knee was drawn up towards the chin. If allowed to remain so the spasms occurred.

Two months after the accident, on exposure to a draught from an open window, violent convulsive rigors set in, followed by perspiration sufficient to saturate the bed, while vomiting and retching were almost incessant. Soon after, white, and sometimes greenish, masses of purulent matter were found at the orifice of the urethra, and then, for many weeks, pus was diffused through the urine.

She progressed so favourably that in July she walked a little. A stitch and dragging feel gave hope that the kidney was attached somewhere. It could not be distinctly felt by the hand. In November there was a sub-acute attack of inflammation, and the tumour was apparently loose again—the loosening, no doubt, aided by a cough. Horizontal rest, as before, was enjoined till February, 1873, when no movement was experienced. About this time great pain was complained of in the lumbar spine. For this Dr. Elliott recommended a spinal support, which was of great use. It enabled her to sit up, and soon to drive out. The kidney has remained motionless ever since, and she is now well and active, only feeling pain after indigestible food, or a second glass of wine. The robust state of health to which she has attained prevents much examination. On pressing the fingers firmly towards the spine, a little



above the crest of the ilium, a tender spot is reached—but there is no definite outline of a tumour to be felt, nor is it to be expected that much of the gland remains, although the pain and a cloudy state of the urine after indiscretion in diet, show that it has not all gone. The cloudy urine might depend on sympathetic irritation of the other kidney, which had shown itself before on several occasions.

Leaving out the recorded cases of movable and floating kidney, and of kidneys displaced by tight lacing and violent vomiting in relaxed habits, very little is found in books of any other form of luxation. This must, therefore, be looked upon as a rare case. Some have gone almost as far as to deny its possibility to me, but the abscess fortunately came, and set the matter at rest. I say fortunately, because, although a dreadful remedy, it has been followed by very firm and satisfactory adhesions, without which strangulation of the vessels, and its consequences, were to be apprehended.

During this lady's illness, opiates and anodynes were largely used. Chloral hydrate was the most satisfactory one. Morphia always deranged the stomach, even when used in a suppository. Very mild unstimulating food only could be borne. A mutton-chop or a glass of wine irritated the inflamed kidney almost as much as they would an inflamed stomach.

The PRESIDENT said he had listened with great pleasure to this interesting case. He scarcely thought there would have been so favourable a termination of it.

DR. FOOT said, if he understood Dr. Peebles rightly, he referred the dislocation of the kidney to the result of the accident. He found it more easy to believe in the movability of a kidney after such an injury than without it. He was of opinion that the diagnosis of movable kidney was much more frequently made during life than verified *post-mortem*. The latter part of Dr. Peebles' remarks struck him as interesting—namely, the immediate effect of a too highly nitrogenous diet upon what probably was the sound kidney. This bore out the experience of most of those who had attended closely to the feeding of convalescents after scarlatina. The irritability of the kidneys was largely increased by the administration of unsuitable food. This was at once perceived by albumen in the urine, and thus renal disease might be excited, which probably, would otherwise have remained in abeyance.

DR. M'SWINEY asked, whether any blood had been observed in the urine? He should expect to find it after an accident capable of exercising such an injury on the renal organ as to disinter it.

DR. H. KENNEDY said, that it seemed to him the case just detailed was

more easily explained under the idea that the case had originally been one of floating kidney; which state, though still questioned by some, there could be no doubt did sometimes exist, as was proved before the Pathological Society of London. The reason he held this view of the case was, that any one who ever took out a kidney in the dead subject could not but have observed the very firm way it was attached in its berth, and it was very difficult, if not impossible, to conceive an amount of violence which could dislocate the organ, without at the same time causing a fatal result. As to the ultimate termination of the case, though he hoped it would be favourable, he would still be very cautious in saying so. For cases like the one stated—that is, where suppuration in and about the kidney occurred—might appear to be quite well, and yet were very apt to burst out again, even after a long interval. They were essentially strumous cases, as was most probably the case which had been just detailed.

DR. FINNY said it had been his fortune once to meet with a case in which he had no doubt there was a floating kidney. There were many signs which made him believe it to be a floating kidney, and he should like to know whether they existed in Dr. Peebles' case. He could not understand how an organ placed in so secure a position as the kidney could be displaced by an accident. The points he observed in the case to which he referred were these:—There was a perfect absence of dulness on percussion over the region where the kidney lies, while the kidney was floating towards the epigastrium; but when it was replaced in its normal position dulness returned: when the patient lay on his left side the dulness disappeared from the lumbar region, and the clear sound on percussion was pronounced. He should like to know whether in Dr. Peebles' case there was complete displacement of the organ. The signs Dr. Peebles had described might be easily accounted for by other causes. Unless there was evidence that the kidney was thoroughly disorganized by the discovery of a large quantity of pus and blood, he could hardly understand how such an organ could undergo disorganization under the circumstances described. It was quite possible that pyelitis, leading to atrophy of the kidney, might be produced, but he could hardly understand how displacement of the organ could have led to this.

DR. YEO agreed with Dr. Foot, that it was easier to conceive the displacement of a kidney by violence than that the organ could be dislodged without any injury having been received. He had seen a case of this kind in a labouring man, forty-five years of age, who had recovered from serious injuries caused by a cart passing over his loins. A year and a half after the accident his left kidney could be displaced by manipulation over the margin of the quadratus muscle, and could be

seized through the abdominal wall, as it lay under the ribs. The patient could also change it from its normal bed to this position by a wriggling motion of his body. Firm pressure on the tumour produced symptoms similar to those caused by passing a renal calculus, and not one of the many physicians who saw it doubted that it was the kidney.

DR. PEEBLES, in reply, said it was very remarkable, how the least particle of improper food affected the kidney in this case. With regard to Dr. M'Swiney's question, as to the presence of blood in the urine, he did not see the case until three months after the accident, and could not obtain any information on the subject. Many views had been put forward as to the existence of floating kidneys. The late Professor Smith told him he did not believe in such a thing, and, at all events, he had never seen one. He was certain this was not a floating kidney before the accident. He accounted for the displacement by inflammation taking place in the cellular tissue, which loosened the kidney, and he did not think it came out for a month or three weeks after the accident. In reference to the remarks of Dr. Kennedy, he might say, it had all the appearances of a strumous abscess.

The Society adjourned.

# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

---

THIRTY-SIXTH ANNUAL SESSION.

---

*Saturday, 14th March, 1874.*

LOMBE ATTHILL, M.D., Vice-President, in the Chair.

*An Anomalous Labour.* By CHARLES DRUMMOND MOUTRAY, Licentiate Royal College of Surgeons, Ireland; First Midwifery Prizeman, Steevens' Hospital.

GENTLEMEN,—On the sole ground that the case I present is one not unworthy of record, I rest my defence to the charge of precocious presumption in addressing this assembly of my seniors.

Mrs. D. entered her third labour during the evening of the 18th August, 1873; on the morning of the 19th I saw her.

The previous history is briefly this:—First labour—Pregnancy favourable; labour tedious; delivery natural; child survived but a moment; convalescence favourable; attendant, a qualified practitioner. Second labour—Pregnancy favourable; labour tedious; delivery natural; child survived; convalescence slow and incomplete; attendant “a midwife,” of whose skill I possess a record in my patient’s remark: “That horrid woman destroyed me.”

I beg now to pass to the present pregnancy:—The health had been tolerable, and her full time had been reached. She appeared a healthy young woman, and of the existing labour informed me that pains had continued, in rapid succession, all through the night—but she graphically described their severity as “only just enough to keep one awake”—she further assured me that the abdominal protrusion was “larger and more pointed” than on either previous occasion. To account for this I found the womb displaced, the fundus forming a conical projection behind the umbilicus, the os uteri inaccessible through the vagina. Confinement on her back quickly remedied this. I then found the os flabby and very dilatable, and, as the quantity of the liq. amnii was obviously abnormal, I meditated artificial rupture of the membranes to remove the over-distension to whose charge I laid the weak uterine action.

I first passed a catheter, and was preparing to supplant the binder I had applied to aid in rectifying the uterine mal-position, by my hand, lest the sudden evacuation of the womb might lead to untoward results, when



the circumstance occurred which lends whatever interest this case may claim.

The atrophy of the abdominal uterine parietes was extreme—a single tightly-drawn membrane seemed the sole partition between my hand without and the fœtus in utero.

A spasmodic pain, of whose force the most exacting could hardly complain, seizes the patient; she starts bolt upright, and the discharge of the liq. amnii takes place spontaneously; my hand rests gently over the abdomen, and, as the womb empties itself, I feel the viscus give an odd jerk downwards and forwards. What I now discover to have occurred is this:—The head has fallen downwards in front of the pubic symphysis, carrying before it a pouch from the anterior wall of the womb; the bladder has been bent downwards and forwards, and lies between the mons veneris behind, and the fœtal head, in its covering of the uterine and abdominal parietes, in front. I at once have the recumbent position resumed, and demand a consultation, to which request I now receive a stout refusal, though, as will hereafter appear, I gained this all-important point at a later period in the case. I place, and continue all through, the support of a hand below and in front of this pouch, and proceed to an accurate vaginal examination. I acquire the following details:—The os uteri flabby; divided by the superior aspect of the pubic symphysis into an anterior portion, which forms the superior margin to the posterior wall of the uterine pouch, and a posterior portion in vaginam. Originally a face to pubis case, the occiput is now the nearer to the pubis, the fœtal neck lies on the symphysis pubis, and in front of it the superior dorsal region presents. I first try the united pressure of two fingers placed at the back of the neck, per vaginam, and the right hand making suitable efforts on the front of the pouch externally—utterly useless. I now meditate version, either laterally, so as to tilt the head upwards, or in its ordinary acceptance, but abandon both as likely to ensure a danger I already dreaded—rupture of the uterus—which appeared now to be as diligent as it had been slothful at first. I now adopted a manœuvre suggested to me by the existing difficulties, to have applied the forceps after the recognized fashion would, it is clear, have been impossible, but taking one of the blades directing its concavity downwards, I slid it from behind forwards over the pubic symphysis, first placing a warmed catheter, without stilette, in the urethra to act in the double capacity of a guide to the pouch and a danger signal to avoid the urinary passage. With this lever I now tried to tilt the face from in front upwards by depressing the occiput from above, and, as it were, stretching the back of the neck; this was beyond the power of the force I deemed it permissible to exercise, and, therefore, I reversed my mode of attack. Sliding the tip of the forceps' blade under the occiput—that is, below and behind it in its present position, and thus reaching the posterior fontanelle, I succeeded in tilting the

occiput upwards, aiding my efforts by pressure on the back of the child's neck, and taking care to avoid the fontanelle, not to damage the maternal urinary bladder or passage, and to make a fulcrum of my own fingers only. Perseverance in these efforts at length brings the tip of the occiput resting on the symphysis pubis. The head is evidently intensely ossified. I now apply the forceps in orthodox fashion, but from behind forwards, and, after a prolonged coaxing of the blades, I effect their happy union; raising the head and drawing it backwards I increase the advantage gained, but, beyond a certain point, my utmost efforts fail to bring it. I now try to reach the neck, that by drawing it downwards I may remove the wedge-like shape the anterior portion of the cranium now forms, in this I fail, and after hours of anxious labour to avoid its use I find myself face to face with the "dernier ressort," the "forlorn hope"—craniotomy.

The preparation of an elaborate remonstrance to her objections to consultation, pointed out to me a *ruse* which happily effected my object. I suggested chloroform, she greedily demanded the boon, and made no objection to what I assured her was necessary—the presence of a second surgeon during its administration.

My old master, Dr. Isdell, was soon at my side, with his proverbial skill in handling the forceps. I had hoped perhaps the birth might have been effected, but the trial, made at my solicitation, failed to advance the head in the slightest degree, and craniotomy was then reluctantly resorted to, and with difficulty effected. Evisceration narrowly escaped, and the birth of a splendidly developed male child eventually completed. The uterus contracted well, and expelled the placenta. A binder specially adapted to the support of the hypogastric region was, to my surprise, quite successful in preventing the continuance, or recurrence, of the protrusion. The mother made a speedy and perfect recovery, and is once more *enceinte*.

In abler hands I wish to leave the answering of a few points suggested by this case.

Is not the absence both before and since of even an approach to that condition known as "pendulous abdomen" peculiar?

If the primary efforts of the womb owe their debility to over-distension, the result of hydromètre, is not the splendid development of the infant an unusual coincidence?

Could the spasmodic pain to which I have referred have indicated the lesion of the recti abdominis muscles described as sometimes occurring in parturition, and would not their attenuated condition and the subsequent accident seem to favour the assumption, while the discharge of the liquor amnii may have resulted from a simultaneous uterine effort, or the strain resulting from the patient's sudden alteration of her posture?

*On the Use of the Perchloride of Iron in Post-partum Hæmorrhage.* By LOMBE ATTHILL, M.D., Fellow and Examiner in Midwifery, College of Physicians; Obstetric Physician to the Adelaide Hospital; Vice-President of the Society.

THE discussion which ensued on the publication of my paper on "The Anticipation of *Post-partum* Hæmorrhage," in the *British Medical Journal* for November 1st, 1873, has led to the appearance of numerous communications in the columns of that journal, as well as in those of the *Obstetrical Journal*, on the proper method of treating *post-partum* hæmorrhage, more especially with reference to the advisability, or otherwise, of injecting a solution of the perchloride of iron into the uterus in such cases. This question is one of great importance, and I deem no apology necessary for bringing it before the Society to-night.

It is much to be regretted that the discussion which has hitherto taken place on this point has not been carried on with greater calmness. The question to be decided is one to which too great weight can hardly be attached. If the perchloride of iron be a remedy as safe as it is avowedly efficacious, its use should, without doubt, be encouraged. If, on the other hand, its employment be fraught with such danger as is maintained by some, we should, with equal distinctness, reprobate its use under almost any circumstances. This being the case, it is a matter of deep regret that some of the writers who have taken part in the discussion—not yet concluded—should allow themselves to be drawn into the grave fault of substituting assertions for facts, uncourteous denials for proofs, and to forget themselves so far as to indulge in pungent sentences, which, however clever as retorts, savour more of personal antipathy than of scientific argument. Such papers, however agreeable to read, fail utterly in deciding in any way the value of the plan of treatment under consideration, that can be effected alone by carefully recording the cases in which the treatment has been tried, and the results. Such records should state the condition of the patient at the time when the styptic was injected, its immediate effect, and the termination of the case. The points to be decided, in my opinion, are three—

1st. Do cases of *post-partum* hæmorrhage occur which we have reason to believe would terminate fatally, or at least in which life is seriously endangered, notwithstanding the judicious use of the remedies ordinarily employed for the arrest of hæmorrhage?

2nd. Is the injection of a solution of the perchloride of iron of itself a dangerous remedy? and

3rd. If so, are the dangers likely to follow its use such as to outweigh its obvious advantages as an agent capable of effecting with almost certainty, the further loss of blood?

In order to aid in some degree in the elucidation of these most important questions, I shall detail the particulars of the cases in which the solution of the perchloride of iron has been injected into the uterus for the arrest of *post-partum* hæmorrhage in my private practice, hoping that my doing so may induce other members of the Society to bring forward the facts connected with similar cases, and that as a result, some practical inferences may be deduced on which sound rules for future practice may be based.

This I will assume as proved, that the perchloride of iron applied to the interior of the womb is an efficient hæmostatic. To my judgment this much is conclusively proved.

I think the first of the three questions I have put down for discussion will be answered in the affirmative all but unanimously. Dr. Barnes and Dr. Gream state that they have "never seen a woman die from *post-partum* hæmorrhage when under their own care from first to last." I regret to say my practice has not been equally successful. I have lost at least one patient of *post-partum* hæmorrhage, whose bed-side I never left from the termination of the first stage of labour till death ensued, and whose labour, up to the occurrence of the hæmorrhage, which did not set in till after the expulsion of the placenta, was in all respects easy and natural. In her case I exhausted all the ordinary methods employed for the arrest of hæmorrhage, but my patient died before my eyes. I admit, however, that I did not rely on the use of brandy to the extent recommended by Dr. Gream. I gave it freely, both by the mouth and by the rectum. No doubt Dr. Gream would say the fatal result followed simply because "I did not give enough." But if I did not give it by the half-pint, I gave it by the ounce, and the large doses of alcohol I did give, were nearly useless, because they were rejected as fast as swallowed. Moreover, I regret to say, I did not inject the perchloride of iron. It was the first case of severe hæmorrhage which occurred in my practice after that method had been brought under my notice by Dr. Barnes; and, like many of my brethren now-a-days, I feared to use this, to me, new and powerful remedy. I now firmly believe that to this timidity the death of my patient—a young wife and a young mother—was due. I feel that she might still be alive if only I had used a remedy I knew of, but had not courage to employ. This, however, I have to compensate me, that though since then I have stood beside the bed of more than one whose life seemed to me in greater peril than her's, to whom I have just alluded, no such scene as that I then witnessed followed, nor do I believe it ever will again. The lesson I that day learned taught me the utter inutility of the "ordinary means at our command" for the arrest of *post-partum* hæmorrhage in a certain class of cases.

I shall now lay before the Society the particulars of the following five cases which have occurred in private practice. I should add that I have



seen the perchloride of iron used in other cases, but as they were not those of patients directly under my own care, I do not refer at present to them, but shall merely premise that, so far as I am aware, no unfavourable symptom subsequently occurred in any of them.

CASE I.—This was, I believe, the first occasion in which the perchloride of iron was used in Ireland. The patient was the wife of a medical man, who, never robust, suffered much from sickness of the stomach during her pregnancies. She was an example of that numerous class of women whose health is often permanently injured from the habit, frequently I believe acquired, of taking an altogether insufficient quantity of food. Her husband assured me that during the whole of her married life, the quantity of food consumed by her day by day was so small that, to use his own words, “she eat nothing.” During this her fourth pregnancy her appetite, always small and capricious, was further impaired by constant nausea and frequent vomiting. Some hæmorrhage occurred on the morning of the 11th July, 1869. She was then in the eighth month of her pregnancy, but the loss was so slight she did not think it needful to send for me, especially as there were not any pains present. Her nurse-tender, however, calling by chance, she kept her in the house; after a time sharp pains set in, and the labour terminated rapidly in the birth of a dead child. Slight hæmorrhage continued during the progress of the labour, which terminated under the sole charge of the nurse tender. The placenta was expelled almost immediately on the birth of the child. The hæmorrhage, however, continued, and the husband, becoming alarmed, sent for me. I was from home, and the messenger proceeded to Dr. Kidd’s. He has given me the following note of the case:—“The lady lived some distance out of Dublin. Her husband, a medical man, wrote to you saying she had been delivered some hours, and was very weak from the effects of *post-partum* hæmorrhage, which was still going on. As you were from home, the note was sent to me, and I at once visited her, taking with me a syringe and a solution of perchloride of iron, with which I had for some time been provided, in consequence of reading Dr. Barnes’ papers on the use of the perchloride. When I arrived I found the lady in a state of extreme prostration; the surface was cold; she was almost pulseless, throwing her arms about, and begging to be allowed to sit up and get air. A skilful nurse was holding the uterus. For some time after my arrival I took charge of the uterus myself, and found, as the nurse had said, that the moment the pressure was removed the uterus relaxed and the bleeding returned. It was evident she could not bear the loss of more blood, and that there was no time to be lost, so I proceeded to inject the solution of the perchloride of iron as directed by Barnes; this at once checked the bleeding; the uterus contracted and remained firm. I applied a bandage, gave

brandy and opium, got hot jars and flannels to the surface, and watched her closely till reaction was fully established; by this time you had arrived, and took further charge of the case."

The condition of the patient when I saw her fully confirmed the account given by Dr. Kidd; reaction was established, but the patient was still in a most precarious condition. She was greatly exhausted, the exhaustion being kept up by incessant vomiting, which had set in immediately after the birth of the child, and which, notwithstanding the arrest of the hæmorrhage, continued with unabated violence. Brandy, opium, ice—all were tried, and all failed to check this at first distressing but now alarming symptom. Tinct. opii injected into the rectum was equally inefficacious; at last I thought of trying the hypodermic injection of morphia—a remedy I had at that time but little experience of. I injected half a grain of the acetate of morphia; this had an instantaneous effect. She did not vomit once afterwards, and from that moment improved and made a good recovery. I have since then attended this lady in two confinements, both of which were perfectly normal.

As I have already mentioned, this was the first occasion in which the perchloride was used in Ireland for the arrest of *post-partum* hæmorrhage, and I firmly believe that to Dr. Kidd's promptitude in using it, this patient owes her life.

CASE II.—Mrs. C., a pale delicate-looking lady, aged twenty-three, with an anæmic murmur, audible at the base of the heart, gave birth to her second child on the 16th October, 1869, after a short labour of but four hours' duration. The placenta was expelled in about fifteen minutes after the birth of the child. During this interval I kept my hand constantly on the fundus of the uterus, it contracted firmly, and I applied a binder. Shortly after a small stream of blood was observed to trickle down from the vagina. On loosening the binder, I found that the uterus had become relaxed. The application of my hand caused it to contract immediately, and some clots were expelled. This alternate contraction and subsequent relaxation of the uterus, so often seen in the case of women with relaxed muscular tissue, was repeated several times. The total quantity of blood lost was not large, but it told greatly on this delicate and fragile lady. Cold judiciously applied, ergot, friction, &c., failed to produce more than temporary contraction of the uterus. The fatal case of *post-partum* hæmorrhage I have already alluded to was fresh in my memory; it had occurred in my own practice and in a very similar patient but a few months before. The successful result of the case just detailed had lessened, if it had not altogether removed, my dread of the action of the perchloride of iron. My patient was in a critical state—indeed, to one in her state of health, of great danger. I resolved to use the perchloride of iron, and accordingly injected about an ounce of the liquor of the per-

chloride of iron, B. P., diluted by the addition of three ounces of water, into the uterus. The effect was instantaneous—the uterus contracted much more firmly than before, and *did not again relax*. Not a drop of blood was subsequently lost. This lady recovered without a bad symptom, and has been confined twice since.

CASE III.—In July, 1870, I was requested to visit a lady, the wife of a medical man, who expected her first confinement in about a month's time. Her general health was very bad; she had suffered much during her pregnancy from sickness of the stomach, which, though it had now ceased, had reduced her strength greatly. She was unable to eat any solid food, her appetite, such as it was, being extremely bad and capricious. She was very large, and quite unable to take exercise. She suffered besides from severe pain in her back and along the margin of the false ribs. Her complexion was sallow, and her aspect indicated much suffering. I felt anxious as to the result of her confinement. Labour set in during the night of the 1st August, 1870; the pains in the first stage which occupied upwards of ten hours, were short, harassing, and inefficient; in the second stage they were equally inefficient. The head descended slowly into the pelvis, but did not advance further, and finally, after the lapse of fifteen hours from the commencement of the labour, I applied the forceps, and delivered her of a living child. Ergot had been previously given, but sickness of the stomach rendered it useless. Immediately after the birth of the child some hæmorrhage occurred, and I removed the placenta by pressure. Its expulsion was followed by the most copious and alarming hæmorrhage I ever witnessed. The blood gushed out in such a copious stream, that the bed was saturated and the floor deluged with it, in a shorter space of time than it has taken me to pen these lines. My hand all this time was on the uterus. In a few minutes life seemed extinct in this previously debilitated and exhausted woman. As rapidly as I could, and without having recourse to any of "the ordinary means," I injected some ounces of a diluted solution of the perchloride of iron. It acted at once—the hæmorrhage was arrested, and my patient slowly revived. On the third day she was wonderfully well, and continued so to improve till the tenth day. Then she had a rigor, the pulse rose rapidly to 160 in the minute, well-marked symptoms of peritonitis subsequently set in, and she died on the fifteenth day after delivery.

CASE IV.—Mrs. L., a delicate lady, whose health had been very bad during pregnancy, gave birth to her first child on the 9th Oct., 1871, after a labour of eighteen hours' duration, a living child. The second stage occupied two hours. The child presented with the feet. After the expulsion of the placenta hæmorrhage set in, as in case No. II. Notwithstanding the use of "the ordinary means," the hæmorrhage

continued; the uterus would contract, but not firmly, and a constant stream of blood trickled from the vagina. The patient was greatly exhausted, and became very faint. I injected, as in case No. II., about four ounces of the solution of the perchloride of iron, in the proportion of two parts of water to one of the liquor. The uterus instantly contracted firmly, and no more blood was lost. This patient, considering her constitutional delicacy, made a good recovery, and became pregnant again early in last year. Her labour on this occasion forms—

CASE V.—Labour on this occasion set in soon after midnight on the 27th October, 1873. During the whole of her pregnancy her health had been so bad that I looked forward with much apprehension to her confinement. She suffered, from the very commencement of utero-gestation, from constant sickness of the stomach. This was not mere nausea, with occasional vomiting, but almost everything swallowed was rejected. She had besides the most absolute loathing for food. The tongue was always coated with a thick yellow fur. The bowels were alternately obstinately constipated or violently relaxed. She had, in addition, two severe attacks of illness—the one of inflammatory sore throat; the other of inflammation of the ear, terminating in abscess. For weeks at a time she was supported by means of enemata of beef-tea and brandy, and more than once during the course of her pregnancy I seriously considered the propriety of inducing premature labour. Labour set in soon after midnight on the 27th October. The first stage was very tedious, and occupied upwards of twenty hours. So utterly inefficient were the pains that I formed the opinion that the uterus opened by a process of mere relaxation, no appreciable pressure being brought to bear on it by the uterine contractions. As soon as the os uteri was fully dilated I ruptured the membranes, and commenced the administration of ergot—not with the view of hastening the labour, but of preventing the occurrence of *post-partum* hæmorrhage. The drug was, however, vomited. The head did not enter the brim; it appeared simply to rest on it—the short, inefficient pains being altogether powerless to cause its advance. I, therefore, applied the long forceps. This was effected with ease. I extracted very slowly; the operation occupied, on this account, nearly forty minutes. The pelvis was roomy and the child small; no difficulty, therefore, was experienced in extracting the child, which, though very feeble, ultimately lived. The uterus, somewhat to my surprise, contracted well. Mindful of the tendency to *post-partum* hæmorrhage exhibited after her former labour, I had, before I applied the forceps, not only administered ergot in combination with strychnia, but had in readiness, in addition to cold water, &c., a vessel containing two ounces of the liquor of the perchloride of iron, diluted with four of water; but my



precautions seemed needless. The placenta was in due time detached, and expelled, with the aid of gentle pressure applied to the fundus. After its expulsion I still continued for some time to keep up pressure with my left hand on the uterus. As it continued firmly contracted, I then applied the binder, with pads under it, firmly. All seemed so well I thought I might soon with safety leave my patient, when, after the lapse of more than half an hour, she began again to vomit—a copious stream of blood instantly issued from the vagina, and before I could unpin the binder the uterus was so relaxed as to reach above the umbilicus. The pressure I applied seemed only to increase the flow of blood, without exciting any contraction. In five minutes my patient was almost pulseless. This seemed to my mind one of those desperate cases in which I dared not lose time by the employment of what I was satisfied in her case would prove to be inefficient remedies. I, therefore, at once injected about four ounces of the solution I had fortunately previously prepared. The hæmorrhage was at once arrested, the uterus contracted, but so nearly had life been extinguished, that two hours elapsed before the pulse returned with any degree of firmness to the wrist, or that I dared to leave the bedside of this patient; and so great was the subsequent prostration, that for two days she lay in a state of semi-unconsciousness. She swallowed mechanically beef-tea, brandy, &c., when placed to her lips, but never spoke except when roused. The urine was drawn off with the catheter, the bowels did not move, there was not any attempt at the secretion of milk. Her condition, however, slowly improved, no bad symptoms occurred, and she regained in time her usual health. I understand that she is now (March, 1874) again pregnant.

The foregoing five cases occurred in my private practice, and I had an opportunity of judging not only of the previous state of health, but of tracing the subsequent history of each patient. I desire to comment on some points which appear to me of importance, and as calculated to guide us—first, as to the class of cases in which the injection of a styptic into the uterus for the arrest of *post-partum* hæmorrhage is likely to be most useful; and, secondly, as to its subsequent effects on the patient.

1st. It is noteworthy that the only cases which seemed in my practice to demand this treatment were women in a previously bad state of health. Case No. 1 was that of a lady who not only suffered from sickness to an excessive degree during pregnancy, but who for a long time previous to, and of course also during her pregnancy, consumed almost no food, and what she did take was of an improper character. No II. was markedly anæmic. No. III. was in such bad health as to cause much alarm to her friends on this account prior to labour. Cases Nos. IV. and V. were the same patient. She, too, was on both occasions in a very bad state of health—so bad, indeed, that the induction of premature labour seemed more than once demanded. In all it may be fairly assumed that the

blood was in an abnormal condition, probably destitute of its proper proportion of fibrine. This seems specially likely to have been so in Case II., in which, though the uterus contracted fairly, the hæmorrhage continued.

2nd. As to the results:—In three of the four patients pregnancy subsequently ensued; this fact proves clearly that the injection of the perchloride of iron in no way injured the uterus.

In four of these five cases, notwithstanding previous bad health and the great loss of blood sustained at the time, no unpleasant symptoms of any kind subsequently presented themselves. In one case death ensued. Taking into account her previous ill-health and the acknowledged tendency which always exists to the occurrence of peritonitis after excessive losses of blood, it hardly seems a reasonable inference that in her case death was due to the effects of the injection of the styptic. The Society have, however, before them all the facts which I am possessed of, for no *post-mortem* examination was possible. My own opinion is that this patient would probably have died whether the perchloride had been injected or not. Pyæmia, phlebitis, and peritonitis have, as is well known, carried off numbers of patients who have suffered from *post-partum* hæmorrhage, long before the injection of a styptic for its arrest was proposed, the debility resulting from the loss favouring the occurrence of these forms of disease; and in the case of the patient under consideration, the state of her health previous to labour aggravated the danger, to which all cases of hæmorrhage are liable. But even were it proved that her death was the result of the use of the perchloride, a further question has yet to be decided—namely this, believing as I did and still do, that this patient would have died from hæmorrhage, and that in a few minutes, was I justified in using an agent which alone, in my opinion, was capable of saving her life? supposing it to be proved that in a certain proportion of cases the use of that remedy would be followed by fatal results.

This question seems to me to be identical with that which is involved in deciding on all capital operations, notoriously in that of ovariectomy, and that it must be decided on the same principles. I shall not, therefore, discuss it further.

For myself I have arrived at the following conclusions:—

1st. That cases of *post-partum* hæmorrhage occur in which the injection of the perchloride of iron, or some similar styptic, is alone capable of arresting the hæmorrhage.

2nd. That the injection of such styptic does not necessarily increase the tendency which exists in such cases to the occurrence of pyæmia, septicæmia, or peritonitis.

3rd. That this treatment is specially applicable to anæmic patients.

4th. That while it should never be had recourse to unnecessarily, it should not, on the other hand, be delayed too long.

I may add that in using the solution of the perchloride of iron, I carry out in the main the directions given by Dr. Barnes. I have not, however, in any case injected more than six or eight ounces, sometimes as little as four ounces of the fluid. I also use it somewhat stronger than he does, namely, in the proportion of one part of the strong liquor, B.P., to two of water. The important point in using it is to take care that the end of the tube is passed up to the fundus of the uterus, and that the fluid be injected slowly. I should add that I have not met with any case in which the uterus did not immediately contract firmly on the perchloride being injected. I am inclined to attribute this to the fact that I had recourse to the remedy before the powers of the patient were so exhausted as to render the uterus incapable of responding to the stimulus.

Before concluding this paper, I desire to say a few words as to what are "the ordinary means" employed for the arrest of *post-partum* hæmorrhage. They are the administration of ergot, pressure and friction on the fundus of the uterus, and the application of cold; these, with the addition of the free exhibition of brandy or other stimulant, may, I think, be considered the means ordinarily had recourse to by practitioners.

As to ergot, it is a most uncertain agent, and while most useful if administered some time before the occurrence of the hæmorrhage, is, in my opinion, seldom of much value if given after it has set in. Ergot takes at least twenty minutes to act, and besides is often in these cases vomited. Injected hypodermically it is, I believe, capable of doing much good, but its irritating properties, when thus used, render this method of employing it not altogether unobjectionable. I am at the present time engaged in endeavouring to obtain an efficient and, at the same time, unirritating preparation of this drug for hypodermic injection. As yet I have been unable to obtain any definite results.

The value of pressure on the fundus, if it be judiciously applied, can hardly be over-estimated; but the most carefully applied pressure, or pressure combined with friction, will at times fail to check the flow—in spite of all, the bleeding will go on.

Cold, one of the most potent means at our command for stimulating the uterus to contract, is frequently useless—nay, more, often positively injurious in consequence of its being improperly used. To do good it must cause a shock. It must, therefore, be applied suddenly while the patient is warm. Its application should never be unduly prolonged, for if once the temperature of the body be so reduced that cold, no matter how applied, fail to produce reaction, the uterus will not contract, and the hæmorrhage will be in no way checked. Therefore, while I am not prepared to say that cold water should never be poured from a height on the patient, I decidedly object to such a practice, for it necessitates the exposure of a large surface of the body, and the saturating and therefore changing of the bedding. Injecting cold water into the rectum is

generally a safe and often an efficient method of employing cold ; but, to be of use, it requires to be done before the patient has become exhausted. Injecting water into the uterus is, I believe, on the contrary, by no means a safe practice. The advocates of this practice tell us that "the injection (of cold water) should be continued till the fluid returns clear." This procedure is, in my opinion, far from being free from danger. I believe it to be quite as likely to be followed by serious consequences as the injection of the solution of the perchloride of iron, while it is far less efficacious. If had recourse to at all it should be done early, and the quantity injected should be but small. Cold water should never be injected into the uterus of a patient exhausted by excessive loss of blood.

Ice introduced into the uterus or rectum will, if the patient be not too much exhausted, cause the uterus to contract. But how seldom is it possible to obtain ice at the moment, and, if attainable at all, much time must generally elapse before it is at hand, and then it is in general too late to be of real good, for, as in the case of the injection of cold water, to be of use it should be used promptly. If it does not succeed at once, its further use can only be productive of mischief. It therefore cannot, for many reasons, be relied on as an efficient agent for the checking of *post-partum* hæmorrhage.

Of the other means advocated for this purpose, and which cannot be classed among the "usual" ones, the introduction of the hand into the uterus is that which is most frequently advocated. There can be no doubt but that in some cases this treatment has proved efficacious. On the other hand, it certainly cannot be relied on. Thus, to quote a reliable authority, Drs. Hardy and M'Clintock give the particulars of a case which proved fatal from the loss of blood, and in which the hand had been introduced into the uterus. The recorded cases in which this plan has been adopted are so few, and the termination of the case, even when the hæmorrhage has been checked, so uncertain, that no positive inference as to its value can be drawn. But for myself, I have always looked on the method as one not free from danger. The introduction of the hand into the uterus is far from being a perfectly harmless operation. In this opinion I am borne out by the fact recorded by Drs. Hardy and M'Clintock, that Dr. Charles Johnson, the then Master of the Lying-in Hospital, looked on this practice with great disfavour, and Dr. Johnson's opinion carries great weight, at least with such of us as remember that able practitioner, and accurate observer.

My object in making these concluding remarks has simply been to show that the means at our disposal for the arrest of *post-partum* hæmorrhage are far from being reliable, and of what importance it is to add to their number one so powerful as the injection of a styptic solution.



*Post-Partum Hæmorrhage, treated by the Application of the Solid Perchloride of Iron to the Interior of the Uterus.* By A. HILL RINGLAND, A.B., T.C.D., Licentiate of the King and Queen's College of Physicians in Ireland, Assistant to the Masters of the Coombe Lying-in Hospital, &c., &c.

A GREAT question which of late has been occupying the minds of obstetricians in this and other countries is, whether the perchloride of iron applied to the interior of the uterus, as a remedial agent for *post-partum* hæmorrhage, is advisable or not? It is needless for me to enlarge on the vital importance of such an inquiry, for on the decision arrived at, whether it be in the affirmative or negative, may depend, not merely the safety of many of our patients, but also the success or failure of our reputation as practitioners; although the latter is, indeed, of inferior importance.

If there be no subject within the extended fields of pathology and histology, upon whose precise nature the advancement of medical knowledge has thrown more light than hæmorrhage, there are assuredly no organs in the body that have derived more benefit from this circumstance than those subservient to reproduction in the female, which, both from their organization and from the nature of their peculiar functions, are more frequently than any other the subject of hæmorrhage.

Dr. Barnes, in his lectures on obstetric operations, has so fully entered into the history of perchloride of iron as a uterine hæmostatic, that nothing here need be said about it. It may, however, be worthy of observation, that he invariably used the styptic in the form of solution.

Dr. Snow Beck and others have taken exception to the employment of the drug: some to its being used under any circumstances, deeming it capable of absorption, and consequently liable to produce pyæmic poisoning; others, on the ground of it being forced into the uterine sinuses, or up the fallopian tubes, and thus effecting irreparable damage to the system of the patient.

Dr. Barnes, the one to whom all credit is due for its introduction into obstetric practice, apprehends that, reasoning from general knowledge, it may, in the form of injection, be not entirely free from danger, for he states that the *fluid* may enter into the circulation, and cause thrombi in the blood vessels; air may be carried into the uterine sinuses, and transferred from thence to the heart; and the injected fluid may run along the fallopian tubes, and escape into the peritoneal cavity—the occurrence of any one of which dangers must necessarily imperil the patient's life. Dr. Atthill, in his admirable communication just now read before the Society, strongly advocates the use of the drug, but he too has used only the fluid preparation.

To the employment of iron in a different form from that hitherto recognized, I am indebted in a great degree to accident, and to the fertile ingenuity of Mr. William Ormsby Wier, the resident medical officer of

the Coombe Hospital. In January, 1872, an extern midwifery case, residing at some distance from the hospital, having arisen, whereir. it appeared to my judgment, that the application of the perchloride of iron was immediately demanded, and neither myself nor my assistants being supplied with the ferric solution, or the apparatus for its employment, Mr. Wier suggested the feasibility of introducing with the hand a small piece of the *solid salt*, some of which he had with him, and painting therewith the bleeding surface of the uterine cavity. The effect was instantaneous—the hæmorrhage stayed, the uterus contracted, the vitality returned, and no untoward consequences resulted. In fact, so satisfied was I at the happy and favourable termination of the case, so rapid were the introduction and action of the remedy, so certain was the knowledge obtained during the performance of the manual operation, that in each and every one of the cases about to be briefly submitted to you I have adopted the same line of treatment, and with what result the sequel will show.

Since January, 1872, there have been in the intern and extern practice of the Coombe Hospital, no less than 23 cases where the use of the perchloride of iron to the interior of the uterine cavity was rendered absolutely necessary. This to some may seem a large number, but I would have it remembered that since I became connected with the hospital, in October, 1871, there have been attended in the institution itself and in the extern maternity connected therewith over 4,500 cases. In this large number it need not be surprising that hæmorrhage has necessarily resulted in many instances; more especially in the extern department, which included nearly 3,500 of the whole. These latter cases frequently meet at the hands of inexperienced students treatment not always desirable, and the patients themselves are in many instances the victims of drink, starvation, and all kinds of wretchedness. The number of cases therefore needing the use of the iron, when compared with the total number of cases delivered—23 out of 4,500—is not so very large as it may seem at first sight.

The brief particulars of each case I now append, arranged in chronological order.

From this table, you will perceive that out of the 23 cases in which the perchloride of iron in a *solid* form was used, 14 *recovered well*, by which I mean that from the completion of their labour to the eleventh day their convalescence was uninterrupted.

*List of Cases.*

No.	Date	Name	Age	Length of Labour	Delivery of Third Stage	How soon Hæmorrhage occurred	Result and Remarks
1	Jan. 3, 1872	Mary Doyle	27	14 hours	*Natural	2 hours	Recovered well.
2	Feb. 15, "	Christine Murray	23	11 "	Placenta adherent	$\frac{1}{2}$ hour	Recovery tedious; had constitutional syphilis.
3	" 21, "	Rose Purcell	26	22 "	Natural	2 hours	Died of phlebitis on 9th day; had advanced pulmonary phthisis.
4	Mar. 15, "	Jane Murphy	22	16 "	Adherent Placenta	2 "	Recovered well; this patient was only eight months pregnant when labour set in.
5	" 18, "	Jane O'Connor	18	19 "	Natural	2 "	Recovered well.
6	April 3, "	Ellen Ormsby	26	17 "	Do.	$\frac{1}{2}$ hour	Recovered well; this patient was delivered of twins.
7	" 11, "	Margaret Seery	28	—	Do.	$\frac{1}{2}$ "	Recovered well; this was a case of partial placenta prævia; delivered by version.
8	June 29, "	Julia O'Neill	30	18 "	Do.	2 hours	Recovered well.
9	Aug. 16, "	A. Joyce	28	21 "	Partial adhesion	$\frac{1}{2}$ hour	Recovered well.
10	Nov. 12, "	Rose Doolan	29	48 "	Do.	$\frac{1}{2}$ "	Died on 10th day from phlebitis.
11	Dec. 16, "	Rose Hall	30	9 "	Do.	$\frac{1}{2}$ "	Died on 9th day; was an unfortunate in very bad health.
12	" 19, "	Lizzie Wilson	21	6 "	Natural	2 hours	Recovered well.
13	" 20, "	Kate Lynch	25	23 "	Adherent Placenta	1 hour	Died in three hours; transfusion tried but failed.
14	Jan. 2, 1873	Eliza Fleming	20	10 "	Natural	2 hours	Tedious recovery; threatened cellulitis.
15	April 13, "	Sarah Bryce	36	12 "	Do.	$\frac{1}{2}$ hour	Died in two hours; had accidental hæmorrhage at full period; transfusion.
16	May 20, "	Margaret Hayden	22	15 "	Do.	$\frac{1}{2}$ "	Recovered well.
17	Aug. 21, "	Margaret Williamson	24	18 "	Placenta adherent	$\frac{1}{2}$ "	Recovered well; a case of twins.
18	" 27, "	Mary Walsh	35	18 "	Natural	2 hours	Recovered well.
19	Sept. 18, "	Bridget Bawrick	30	35 "	Placenta adherent	$\frac{1}{2}$ hour	Recovery tedious; seized with cellulitis on 10th day; delivered by forceps.
20	Nov. 29, "	Mary Hogan	20	23 "	Do.	$\frac{1}{2}$ "	Recovered well.
21	Dec. 5, "	Eliza Cleary	25	24 "	Do.	1 "	Recovered well.
22	" 23, "	Frances Reilly	30	17 "	Do.	$\frac{1}{2}$ "	Recovered well.
23	Feb. 2, 1874	Kate Reilly	26	21 "	Complete adhesion	$\frac{1}{2}$ "	Died on 16th day; a portion of placenta could not be removed; delivered by forceps.

• By the term "Natural" is meant that the third stage was completed within a reasonable period by the natural efforts, and unaccompanied at the time by hæmorrhage.

Three made *tedious recoveries*, one of which (Case No. 2 in the table) was caused by constitutional syphilis, a disease debilitating in the extreme, and in every way militating against the patient's speedy return to good health; while the remaining two (Nos. 14 and 19) were threatened with pelvic cellulitis, but ultimately did well, the first being convalescent on the eighteenth day, the second on the twenty-first. It is clear that this condition can be attributed in neither of these cases to the employment of the iron, inasmuch as the experience of every obstetric practitioner demonstrates, that pelvic cellulitis more frequently results from the manipulation and other treatment necessary to control hæmorrhage than from any other cause.

The further analysis of the table shows, that of the 23 cases therein detailed there had been *six* deaths. From this number must be deducted two cases (Nos. 13 and 15) which died—one within two, and the other within three, hours after delivery—in neither of which the employment of the drug could possibly have had any part in producing the fatal result. This leaves but four to be considered.

One, No. 3, died on the ninth day from phlebitis, but she had all the symptoms and physical signs of advanced pulmonary consumption. The experience of every medical man tells us, that phthisis pulmonalis in its advanced stages is apparently arrested during gestation, but that no sooner has delivery been completed, than it again assumes the mastery, and hurries the patient with terrifically rapid strides to a comparatively early grave. Moreover, the tendency of constitutional diseases of this class predisposes the system to phlebotic and other septicæmic attacks. Is it not reasonable to suppose, therefore, that the death in this individual case was the result of natural causes, and not effected in any degree by the employment of the drug.

Another case, the second of the four (No. 23) may also come under the same category, and be freed from the probability of having died from the use of the styptic. In this case a portion of the placenta, which was morbidly adherent over its entire surface, could not be removed. Here we have at once a cause adequately sufficient to account for her death from pyæmia, without attributing it to any other circumstances.

In case No. 10, the third of the four, the placenta was adherent in a partial degree, the hæmorrhage before and following its removal was of the most formidable character, and although promptly restrained by the employment of the iron, the patient at once fell into a most prostrate condition, from which she never rallied. Never was her pulse below 120, never did the functions of the animal economy exercise their rightful sway, fainting fit after fainting fit subsequently occurred, and on the tenth day she succumbed, threatened with all the symptoms of approaching phlebitis. This case surely tells its own tale, and its history removes it from the category of those fatally influenced by the agent referred to.



But one case remains now for our consideration—namely, No. 11. Here the placenta was adherent. She was an unfortunate, and when admitted to hospital was in a most enfeebled condition, diseased alike in mind and body. Her death took place on the ninth day, apparently from the mere sinking of the powers of life, without any distinct disease to which to attribute it.

It is but right to state that a *post-mortem* examination could not be obtained in any of the fatal cases. They were all extern, and to overcome the Irish prejudice against examinations of this kind demanded greater eloquence than I possessed.

It will, I think, be apparent to all, that in each and every one of the last four cases detailed, more than sufficient cause existed for the induction of the diseases which terminated fatally. True, iron has been used in all of them, but because that number died, can we fairly argue that death was the result of its use? I think not! But, for the sake of argument, let us hold that it was so. It must be borne in mind, that out of the 23 cases in which it was used, all, or at least a very large proportion of them, would have died were it not for its employment, every means known to science having been adopted in each case respectively, but without avail. Every patient was running down rapidly, a fatal result appeared impending, and the styptic applied only when she was almost *in extremis*; and yet, of the whole number, only four succumbed after its application. Surely, the rescuing of the larger number was cheaply purchased at so comparatively small a cost, and I feel no hesitation in holding that the first duty of a medical man is to tide over the immediate danger, irrespective of future contingencies; the more especially as such are, as demonstrated in the cases submitted, only exceptional, and—when they do arise—are in many instances amenable to treatment. In fine, to use the words of Dr. Barnes, “to withhold this remedy then from a woman bleeding to death, because it may do immediate or ulterior harm, is at once illogical and wrong. The first pressing duty is to save the woman from dying. The case is, that other means being exhausted, she would die unless local styptics be applied. Where then is the force of the objection, that these styptics may do ulterior harm?”

DR. MORE MADDEN said:—Mr. President,—We are, I think, indebted to Dr. Atthill and to Dr. Ringland in a twofold degree—first, for the very valuable papers they have just read; and secondly, for the opportunity thus afforded to Dublin obstetricians of recording their experience and opinions on a subject which is now engaging such attention elsewhere. No topic could more fitly occupy the time of an Obstetrical Society than the treatment of the most serious complication of childbirth, and by no representative of the Dublin School of Midwifery could the subject have been more ably brought forward than by Dr. Atthill.

To Dr. Barnes the credit is due of having first introduced the use of the perchloride of iron into British midwifery practice, and whatever failures or fatalities may be ascribed by some to this remedy—and I, for one, am by no means prepared to assert that it is either safe or effectual in all cases—still no unprejudiced obstetrician can, I think, fail to acknowledge that Dr. Barnes has thus placed in his hands a comparatively safe, and more generally efficacious, means of arresting severe hæmorrhage after parturition than any previously used for this purpose.

The question now under consideration has very recently been discussed in the *Obstetrical* and *British Medical Journals* with a degree of warmth which appears altogether incomprehensible in what should be a dispassionate interchange of experiences and opinions between men anxious only to arrive at the truth, and concerning a question in the interests of suffering humanity in which all are intimately concerned.

On one side it is asserted that, by the injection of a solution of perchloride of iron in cases of *post-partum* hæmorrhage, two important therapeutic indications are fulfilled, *i.e.*, first, a direct styptic effect is produced, by which the bleeding uterine vessels are sealed up; and secondly, the uterus is stimulated to permanent contraction. On the other hand, it is not only strenuously denied that either effect is produced, but, moreover, it is alleged that there is great danger of forcing the injected fluid through the patulous uterine sinuses into the circulation, or through the fallopian tubes into the abdominal cavity—in one case causing death from embolism, or, in the other, from peritonitis. I have myself had proof that this remedy is capable of producing the curative effects described by Dr. Barnes, and I have not met with any case in it producing the effects ascribed to it by Dr. Snow Beck.

An agent capable of producing such marked effects on the organ into which it is injected as the perchloride of iron, is, however, like every other active remedy, a two-edged weapon, its powers for good or for evil depending on the manner in which it is employed. I have little hesitation in asserting that one reason at least why we do not often witness in this city the ill effects described as elsewhere so frequently following the injection of solution of perchloride of iron in hæmorrhage after delivery, is, simply, that here this remedy is used, and not abused.

I regard it as a misuse of this remedy to employ it in the indiscriminate manner which appears to be now very commonly adopted—that is, “in all cases of *post-partum* hæmorrhage, to the exclusion of all other remedies.” And I entirely agree with Dr. Snow Beck in his condemnation of this malpractice, being daily more and more convinced by enlarging experience that the observations which I made as to the use of the perchloride of iron, in the last edition of the *Dublin Practice of Midwifery*, published in 1871, are correct. In that work I recommended the employment of the perchloride of iron injection in cases of severe

flooding, after the failure of other measures, including the administration of ergot:—"If hæmorrhage continues after the means before spoken of had been tried—if firm pressure on the uterus, the external application of cold water, or cloths wrung out of iced water to the vulva over the pubis fail to check the bleeding, we must inject cold water by the vaginal syringe into the cavity of the uterus. Should this not now stop the loss of blood, we must resort to the powerful styptic recommended by Dr. Barnes—namely, the solution of the perchloride of iron, which may be added to the water injected in the proportion of one part to four parts of water. This injection acts as a direct styptic to the bleeding vessels, and as a stimulant to excite uterine contraction."<sup>a</sup> Now, when I remember the tone in which my recommendation to use the perchloride of iron, *after* any remedy, was spoken of by one of the reviewers of that book, I feel some satisfaction in finding that the value of perchloride of iron, as defined by myself at a time when most exaggerated views were taken of its applicability in all cases of uterine hæmorrhage, is, after an interval of some years, during which a no less exaggerated reaction against its use been led by some eminent practitioners, now generally adopted in almost my words. In truth, hæmorrhage after childbirth so severe as to require the use of perchloride of iron, is, where delivery has been properly conducted, I believe, a very rare accident. Even in cases of women who have been previously subject to *post-partum* hæmorrhage, if due attention be paid to the duration of the second stage of labour, the physiological duration of which can neither be abridged nor prolonged with safety, if ergot be given when the child's head begins to press on the perinæum, and, above all, if due care be taken to secure and maintain an efficient contraction of the uterus by the steady and increasing pressure of the accoucheur's hand over the fundus as the child's body is expelled—a pressure not to be relaxed till permanent contraction is securely effected—if these precautions be taken we shall have comparatively few opportunities of using perchloride of iron in *post-partum* hæmorrhage.

The cases recently reported by Dr. Snow Beck in *The Obstetrical and British Medical Journals* do not, I think, sustain his views as to the danger of injecting solution of perchloride of iron, as only in one<sup>b</sup> does death appear to have been in any way connected with the use of the styptic injection, being evidently cases—one of pyæmia, and the other of the typhoid form of puerperal fever. In reference to these instances in which patients, in whose cases the perchloride of iron was used to arrest hæmorrhage, died from twenty to forty days afterwards of pyæmia or other diseases, I would venture to observe that it is not altogether impossible that if the flooding had not been thus arrested,

<sup>a</sup> The Dublin Practice of Midwifery. Edited by Dr. Thomas More Madden, Senior Assistant-Physician, Rotunda Lying-in Hospital, p. 189. London. 1871.

<sup>b</sup> Obstetrical Journal, February, 1870.



the patients might have died of hæmorrhage; nor, as far as my experience goes, are deaths from pyæmia, or metro-peritonitis, or embolism, or puerperal fever, confined to cases in which the perchloride of iron has been used.

As this question is one which must be decided by experience rather than by argument, I shall now submit the following notes (very hurriedly thrown together only this day) of all the cases of severe hæmorrhage after delivery in which I had seen the perchloride of iron tried in my private practice from January 1st, 1870, to the present time. Of these cases—ten in all—seven occurred in my own practice and three were seen in consultation. In nine the hæmorrhage was effectually arrested by the perchloride of iron after the failure of other remedies; and in one it failed completely. In seven instances the patients recovered perfectly; in three cases the patients died. But I believe the result in these cases had no connexion with the use of the styptic, as in one case death resulted from typhoid pneumonia; in another from secondary hæmorrhage in the course of small-pox; and the third form I believe to have been embolism. The last case is one which Dr. Snow Beck and those who adopt his views on this subject would, I presume, ascribe the fatal termination to the use of the perchloride of iron. But yet this opinion would not, I think, be justified by the facts of the case. For we know that cases of sudden death resulting from embolism, as this clearly was, occurring after delivery, are unfortunately not rare. I have myself reported no less than three such cases, which occurred whilst I was connected with the Lying-in Hospital, and in which no perchloride of iron was used; and these cases are most common after hæmorrhage.

CASE I.—January 21st, 1870, I delivered, by my short forceps, Mrs. C., aged thirty, wife of an officer stationed in the Royal Barracks, Dublin, of a large female living child, her third, after a difficult labour of thirteen hours. She had suffered from severe flooding after her previous labour, and, therefore, as soon as the head entered the pelvic cavity I administered ergot. There was a smart dash of hæmorrhage, however, after the expulsion of the placenta, which was at once restrained by the injection of the diluted solution of perchloride of iron, after which the uterus contracted firmly and she made a good recovery.

CASE II.—Mrs. B., aged forty, first pregnancy, delivered, by my short forceps, of a posthumous small living child, after a difficult labour of about thirty hours, being a considerable time under chloroform. She was in a most distressing condition at the time, having recently lost her husband, and, immediately after the completion of labour, was attacked by profuse flooding—saturating the bed—and all the ordinary methods of arresting this were resorted to, but none appeared to have any influence



in stopping it; on the injection of the solution of perchloride of iron, the bleeding was at last checked, but not till she appeared completely exhausted. A few days later she was attacked by hysterical mania, and subsequently by the typhoid form of puerperal fever, during the course of which I had the advantage of the assistance of Dr. Hayden in consultation, she being the subject of the most malignant type of puerperal blood-poisoning. Nevertheless, she made a complete recovery after an illness of over two months.

CASE III.—Mrs. B., a somewhat delicate looking lady, aged thirty-two, who had had one living child about five years previously, and had, since then, eight miscarriages, generally from the second to the third month, each being accompanied by considerable hæmorrhage; first consulted me in December, 1871, when I found she was suffering from complete retroversion of the uterus; this was replaced, and a Hodge's pessary adjusted. She continued to wear a pessary till March, when she became pregnant, and there being no displacement, the pessary was removed; about six months later I was sent for and found her suffering from bearing down pains, retention of urine, and tenesmus; and, on examination, found the uterus again completely retroverted. Dr. Kidd now saw her in consultation with me, the displacement was rectified, and a large Hodge's pessary applied. She was kept in bed till after the fifth month, when all danger of displacement being over, the pessary was removed, and she was ordered to the sea-side, where she remained in excellent health during the autumn.

On January 23rd, 1873, labour began at 8 45 a.m., the head entered the pelvic cavity at 9 30, and the child, a very large girl, was born at 10 45 a.m., the placenta followed in fifteen minutes. As she had suffered from severe hæmorrhage after the former labour, on the present occasion I administered a drachm of ergot as soon as the head was on the perinæum, and one immediately after the birth. Nevertheless, ten minutes after the expulsion of the placenta, which, I need not say, was not hastened in any way, very copious flooding set in and lasted for upwards of three-quarters of an hour, during which all the ordinary measures were, of course, employed. The patient became pulseless and faint, and ultimately the profuse discharge of blood was arrested by the long-continued injection of the solution of perchloride of iron. Still, though the uterus was now pretty fairly contracted, there was yet a constant and seemingly irrepressible draining of light red blood from the uterus—a very alarming circumstance, as her condition was that of almost complete collapse from the loss of blood. This draining was, however, at last stopped, the jactitation ceased, and her pulse returned, after she was tightly bound up, by the free administration of brandy and opium.

She went on favourably till the seventh day, when she had a severe

rigor. The following day all the symptoms of metritis showed themselves. During the inflammation I leached her, and afterwards, when the disease assumed a typhoid type, I supported her strength by brandy, champagne, nutrient enemata, &c. The uterine symptoms gradually improved, and during their continuance Drs. Denham and Kidd, and, subsequently, Sir Dominic Corrigan, saw her with me. However, after the complete subsidence of all the uterine symptoms, she was attacked by low typhoid pneumonia, of which she died on the twenty-first day after her confinement.

CASE IV.—Mrs. S., aged twenty-two, second pregnancy, was delivered, by the natural efforts, at 3 30 a.m., April 6th, 1870, of a very large female child, after a labour of only six hours. The placenta followed in ten minutes. She had had considerable post-partum hæmorrhage after the first labour—fifteen months previously—and, therefore, I gave her two doses of ergot before the birth of the child and one immediately afterwards. Notwithstanding this the hæmorrhage set in immediately after the expulsion of the placenta, and continued to a most alarming extent, and for upwards of two hours, during which the uterus alternately contracted and relaxed. The woman became exsanguine, pulseless, and almost moribund before the flooding was permanently arrested. All the ordinary means being, of course, meanwhile resorted to; the child was placed to the breast; firm pressure maintained over the uterus; the hand was introduced into its cavity, repeated injections of cold water, and of the solution of perchloride of iron were thrown up, and enemata of brandy and beef-tea administered; at last, however, the hæmorrhage ceased, the uterus contracted sufficiently, and she recovered; her convalescence being, however, very slow. In this case I am unable to say which of the various remedies employed had the effect of arresting the hæmorrhage, though, I believe, that the credit is due to the combined action of all rather than to the influence of any one.

CASE V.—Mrs. L., aged twenty-four, was delivered of her second child, a male, June 28th, 1872. I had attended her in her first confinement, which was natural, a year before, but on this occasion she was upwards of twelve hours in the second stage, from want of uterine action, and I was ultimately obliged to deliver her by the forceps. On the day before delivery she became gradually covered with the marked eruption of small-pox, which was then epidemic in Dublin. After delivery there was no serious hæmorrhage, but on the eighth day after her confinement, I was hastily summoned at 3 a.m., and found her almost moribund from secondary hæmorrhage, by which the bed and bedding were completely saturated when I arrived—a quarter of an hour after I was first called. I administered stimulants largely, and at once threw up a pint of diluted solution of perchloride of iron into the uterus, this produced little or no

effect, and was repeated as uselessly, and within an hour she sank and died.

CASE VI.—I was sent for, in consultation, by Dr. Boyle, of Rathgar, to see a lady residing some distance from town. She had been for some hours in the second stage of labour; the cord was prolapsed, and we delivered her by the short forceps. There being some hæmorrhage between the birth of the child and the placenta, which was morbidly adherent, I introduced my hand to remove it, and found a large sessile tumour growing from the fundus uteri. The removal of the placenta was followed by severe post-partum hæmorrhage, which was completely arrested by the injection of solution of perchloride of iron. The patient convalesced favourably.

CASE VII.—Mrs. T., fourth pregnancy, was delivered March 21st, 1871, of a healthy living child, by Dr. Wyse, with whom I subsequently saw her, in consultation. After the expulsion of the placenta, the uterus relaxed and profuse flooding set in, which we arrested by the injection of the solution of perchloride of iron. I afterwards learned from Dr. Wyse that her recovery was perfectly satisfactory.

CASE VIII.—Mrs. S., an English lady, aged thirty-two, was delivered of her eighth child, a male, January 4th, 1872, after a labour protracted in the second stage, by inertia of the uterus; the placenta was expelled in twenty-five minutes, after which very persistent and alarming hæmorrhage set in, this resisted all other treatment, and was at last arrested by the use of repeated injections of the solution of perchloride of iron, together with enemata, brandy, beef-tea, and tincture of opium. I have since attended this lady without any recurrence of the hæmorrhage.

CASE IX.—Mrs. B., aged twenty-six, was delivered, by the natural efforts, of a still-born female child, after a tedious labour, being about seven hours in the second stage. She had suffered from post-partum hæmorrhage after her first confinement, and on the present occasion had had a good deal of hæmorrhage during the first stage, which I checked by rupturing the membranes. Immediately after the completion of the third stage the hæmorrhage returned to a very considerable extent, and was arrested only by the perchloride of iron injection. Her recovery was rapid and complete.

CASE X.—Mrs. P., third pregnancy, was delivered, by a midwife, July 14th, at 3 in the afternoon, of a living female child. The placenta was retained and considerable hæmorrhage following all the nurse's efforts to

expel it. I was sent for, three and a half hours later. On introducing my hand I found it extensively morbidly adherent to the fundus uteri and removed it; shortly afterwards severe hæmorrhage took place; ergot and cold failing, the perchloride of iron solution was injected, the bleeding was stopped and the patient recovered well.

I have now briefly related all the cases, favourable and unfavourable, in which I have used the perchloride of iron, and I trust that the Society will agree with me in considering that these cases are, on the whole, in favour of the employment of this remedy in some cases of *post-partum* hæmorrhage which resist all other treatment.

DR. M'CLINTOCK said, that although the subject of the use of perchloride of iron in the treatment of *post-partum* hæmorrhage had come under the notice of the Society in a paper read about four years ago by Dr. Roe, still it did not attract the full attention and discussion which the importance of the question demanded; nor had the subject then attained the great proportions which it now assumed, when, so far as regards obstetric practice, it might be looked upon as the prominent subject of the day. It came very appropriately before the Society now, inasmuch as at a very recent meeting the subject of the *preventive* treatment of hæmorrhage had engaged its attention, and been fully discussed. He believed Dr. Barnes had not said too much for the use of the perchloride of iron when he called it a "new power," placed in the hands of the obstetrician for the arrest of *post-partum* hæmorrhage. They already possessed three or four powerful remedies for the suppression of uterine hæmorrhage. One was ergot, another was cold, and another was manipulation of the uterus, whether externally or internally. The last was a most powerful agency, and he thought the remedial value of this internal manipulation had been somewhat overlooked of late years. There was a fourth agent employed occasionally, very powerful and influential, but, from its nature, almost excluded from clinical use—viz., some form of electricity. With regard to the use of the perchloride, there were three practical questions to be considered. One was its feasibility. Electricity was a very powerful agent no doubt, but in practice it was not feasible, for no man went to a midwifery case with an electric machine in his pocket, and if hæmorrhage suddenly came on there would be no time to send for one. Now, the use of the perchloride of iron was quite feasible; it occupied little or no space in the midwifery bag, and did not require any special instrument for its administration, which could be very easily effected. The next question was as to the *efficacy* of the perchloride in restraining the hæmorrhage, and he thought there could be little doubt about its power to produce coagulation in the mouths of the vessels, and also to induce contraction of the womb, which



was the great means whereby hæmorrhage from the uterus was to be controlled. The third question concerned its safety. He agreed with what had fallen from Dr. Atthill and Dr. Ringland on this subject. It was very well known that women who had suffered a severe loss of blood in parturition were peculiarly exposed to some form of septicæmia, or peritoneal inflammation. Now, in those cases where there had been enormous flooding, if the use of perchloride of iron was followed by peritonitis, it would be hardly fair to set it down to the action of the styptic. The only case he knew where he thought that death could be fairly attributed to the use of the iron injection was one narrated before the Obstetrical Society of London, by Dr. Bantock, where the iron was used, not to stop hæmorrhage, but to prevent it. Its employment was immediately followed by most intense pain, which continued until the patient's death, a few hours afterwards. It was possible that in this case some of the fluid had escaped into the peritoneal cavity. In the other cases, where the injection was followed by different forms of septicæmia, or inflammation, he did not think it fair to set it down exclusively to the iron. There had been a good deal of difference as to the strength of the solution used by Dr. Barnes and other practitioners. He (Dr. M'Clintock) used a solution of about the same degree of strength as the *liquor ferri perchloridi* of the Pharmacopœia, which was a stronger solution than that usually employed. In Dr. Ringland's cases it was the solid perchloride that was used, carried by the hand into the cavity of the uterus, and rubbed over the bleeding surface. He (Dr. M'Clintock) did not think that these cases ought to be placed in the same category with those where the perchloride solution was simply injected into the uterus. It might be well contended, that the arrest of the hæmorrhage in Dr. Ringland's cases was due to the powerfully stimulating action of the hand on the uterus. The utility of the introduction of the hand into the uterus and the effect it was capable of exciting as a remedy for hæmorrhage was too often forgotten. Dr. Collins, in his practical treatise on Midwifery, had laid the greatest stress on its value as a mode of exciting uterine contraction in these cases of *post-partum* hæmorrhage. With regard to the administration of the perchloride injection, he attached importance to two points—viz., to carry the point of the tube well up to the fundus of the uterus, and to have the uterine cavity as free from blood as possible, so as to insure the styptic fluid coming in contact with the interior surface of the organ. In his own practice he had employed this remedy in four cases: in *three* it was completely successful in stopping the hæmorrhage; and in one it failed, though twice resorted to, and the lady died of hæmorrhage. Symptoms of metro-peritonitis ensued in one of the cases, but yielded to the treatment employed, and the patient recovered. This was a case of placenta prævia, in which he had to deliver the child by turning, but the patient sustained an enormous loss

before this was effected. The recurrence of hæmorrhage after the extension of the placenta necessitated an immediate resort to the styptic injection, and its immediate good effects were very striking, though it was followed by very severe pain in the belly. In one of his cases the hæmorrhage followed abortion about the fourth or fifth month, and could not be restrained by the most careful plugging of the vagina. Here also the perchloride injection was successfully used, and without any unpleasant effect. On several occasions where much or prolonged bleeding followed upon miscarriage, he had found good results from mopping the interior of the uterus with the perchloride solution.

On the motion of Dr. M'Swiney the debate was adjourned.

## PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. LYONS, President.

DR. BENNETT, Secretary.

*Colloid Carcinoma of Ovary.*—DR. KIDD said the following case was involved in a great deal of obscurity, and though in many features the diagnosis made was correct, it was not complete, and he thought it therefore the more important to bring it under notice. A lady from the west of Ireland called on him on the 22nd of November last, to consult him as to a tumour in her abdomen. On examination he found that the tumour filled the lower part of the abdomen, and extended about two inches above the umbilicus, and that the abdominal walls were lax and pendulous, allowing the tumour to hang very much forwards. There was a hernia at the umbilicus, which was reducible, and when reduced an opening in the tendon could be felt, through which the hernia escaped, a small oval opening about half an inch in its longest diameter. The walls of the hernia were found to consist apparently of integument only. The contents of the hernial tumour were fluid; placing a candle opposite, and putting the end of his stethoscope on the tumour and looking through it, he could clearly see right through the tumour, and there was no solid matter to be observed within it. The abdominal tumour fluctuated very freely. Persons standing at a distance could see the wave passing through it. There was a solid, very movable mass in the fluid, which, in size and outline, gave some countenance to the idea the patient herself entertained that she was pregnant, but if so, as the uterus was empty, it should be an extra uterine fœtus. The wave of fluctuation did not extend to the contents of the hernia. The tumour was dull all over on percussion, the epigastric region was clear, and also the right lumbar region. The left lumbar region also gave out a clear sound, but to obtain this it was necessary to percuss far back round towards the spine. On examination, *per vaginam*, the uterus was found movable, empty, and a little pushed forward. The tumour could be felt in the posterior *cul-de-sac* of the vagina, but it did not lie low down in the pelvis. The uterus was three inches in length, and when the solid mass in the abdomen was moved, the sound being in the uterus, the handle moved a quarter of a circle. The diagnosis arrived at on this examination was, that there was a multilocular tumour of the left ovary, with a considerable amount of solid matter in it. That it was attached to the uterus was inferred from the movement of the sound. The nature of the umbilical protrusion was not so evident.

It might be ascitic fluid or a portion of the ovarian tumour. The thinness of its walls and the wave of fluctuation not passing through it cast a doubt on its being part of the ovarian tumour. On the other hand, it did not vary in size with the position of the patient, and the clear note on percussion of the lumbar region when the patient was lying on her back, showed that there was no free fluid in the peritoneum, and if any it must be limited by adhesions. The woman was thirty-six years of age, she had been married twelve years, had had two children, the last of which was born nine years since, and she had had no miscarriages. Menstruation had always been regular, both as to time and quantity. On the 4th of December, 1872, she began to think she was pregnant; she had fainting fits, and imagined from the peculiar sympathetic feelings that she had quickened; she had been obliged prior to this to let out her dress, so, though she had not discovered any tumour, there was evidently some enlargement of the abdomen. Menstruation, however, still continued up to the 10th of March, when it ceased. On the 10th of August she again menstruated, but very scantily. She still believed that she was pregnant, and had made all her arrangements for her confinement. However, as the time passed on when she thought her labour should have set in, she consulted her medical attendant, who pronounced that it was an ovarian tumour and not pregnancy, whereupon she came to Dublin. As already mentioned, it was on the 22nd of November Dr. Kidd first saw her. He again saw her on the 25th of November; at that time she was labouring under a choleraic attack, or what was called a bilious attack, purging and vomiting, and that continued for some hours, after which she suffered pain over the whole of the abdomen, which became sensitive to pressure. After this the tumour increased rapidly. It reached, when first seen, not more than about two finger-breadths above the umbilicus. It now rapidly distended the whole abdomen, and the tumour was so tense that the solid matter that had been lying in it could no longer be distinguished. The fluctuation, however, was still very evident throughout the tumour. On the 4th of December the abdomen was measured. The circumference at the umbilicus was 47 inches, at the crest of the ilium  $46\frac{1}{2}$  inches, from the ensiform cartilage to the umbilicus  $8\frac{1}{2}$  inches, and from the umbilicus to the pubes  $13\frac{1}{2}$  inches. Naturally the umbilicus lies about an inch nearer to the pubes than it does to the ensiform cartilage, but here it was 4 inches further from the pubes, showing that the enlargement existed chiefly in the lower part of the abdomen. The tumour continued still to increase, and the woman suffered great distress from pain at the epigastrium, and from restlessness and sleeplessness. She maintained that up to this time her health was perfect. She had never had pain, except from the stretching of the muscular walls of the abdomen since the tumour increased, and all her functions were naturally and regularly performed. On the 6th



of December the tumour had so much increased in size that it was determined to tap it. Accordingly, he tapped her midway between the umbilicus and the pubes with an aspirateur, and drew off about eight pints of fluid, emptying one of the cysts of the multilocular tumour. The effect of this was to diminish the size of the lower portion of the abdomen, but not the upper. The circumference at the umbilicus had been  $47\frac{1}{2}$  inches; after tapping it was  $44\frac{1}{2}$  inches; at the crest of the ilium, formerly  $46\frac{1}{2}$  inches, it was now 42 inches; from the ensiform cartilage to the umbilicus, the measurement, formerly  $8\frac{1}{2}$  inches, remained at that size after tapping; from the umbilicus to the pubes, before  $13\frac{1}{2}$  inches, it was now  $12\frac{1}{2}$  inches. The other measurements remained nearly the same, except that from the umbilicus to the left anterior superior spine of the ilium, which was reduced from  $11\frac{1}{2}$  inches to  $10\frac{1}{2}$  inches. The reason why it was determined to tap on that occasion was that the umbilical tumour began to bulge out at one point, at the upper portion, and gave every indication of being about to burst. He selected the situation mentioned because, if they came eventually to remove the tumour, it would lie in the line of incision, and cause less difficulty subsequently, and they hoped tapping the tumour would have prevented bursting. On the morning of the 12th of December the umbilical tumour burst, and there was a discharge of a large quantity of serous fluid from it. This reduced the size of the upper portion of the abdomen in some degree. The circumference at the umbilicus was further reduced to 40 inches after the rupture. The distance from the ensiform cartilage to the umbilicus remained at  $8\frac{1}{2}$  inches. The fact that the tapping had lessened the bulk of the lower part of the tumour, while the whole mass did not sink lower in the abdomen, led to the belief that there were considerable adhesions between the upper part of the tumour and the abdominal walls; and the diagnosis at this time was that there was a multilocular tumour with a large mass of solid matter in it, that there were considerable adhesions anteriorly and superiorly, and to the uterus, but whether posteriorly or not could not be ascertained. The fluid that had escaped from the umbilicus, when the rupture occurred, was thin and serous, and of this several pints were collected. On the second day it began to be mixed with masses of gelatinous substance, not fluid, but semi-solid, resembling jelly in colour and consistence. Masses of this began to escape at the umbilical opening, and the umbilical hernia itself became dense and doughy to the feel, and it was now irreducible, and scarcely diminished in size. Fluctuation, which had been very distinct and apparent, now became indistinct, and the sensation was more like an elastic boggy feeling than that of fluctuation. The pain at the ensiform cartilage and the epigastrium, which had been so distressing to her, was relieved after the discharge of fluid from the umbilicus, her appetite again improved, and the woman still maintained

that her health, except for the tumour, was perfectly good. The question of operation, as to whether they could remove the tumour, was very anxiously considered. He had the benefit of the assistance and advice of almost all those who, in Dublin, have been in the habit of performing ovariectomy, and all agreed that the case was one in which—if the woman, knowing fully its nature, and the risk she would have to run, demanded an operation—it would be wrong to refuse it. Accordingly, the risks of the case were put before her very fully; she was told that it was very unfavourable for operation, one in which the operation might terminate her life, and that she might die within six hours after its performance. Her reply was that she was aware of the risk, and that she preferred undergoing it to remaining as she was—as she knew she could not live in that state. This being so, on Wednesday morning last he removed the mass. Prior to removing the tumour, he thought it well to explore the umbilical hernia a little more fully, to pass a probe, and so try to learn what it was. He was in some doubt as to whether it was part of the peritoneum with fluid encysted in it, or whether it was a portion of the ovarian tumour. He found he could pass the probe five or six inches into the tumour, directly backwards, showing that the opening communicated with the ovarian tumour. When the operation was proceeded with, the primary incisions were made in the usual way in the linea alba; when he came to the peritoneum he found it exceedingly thick, and was in considerable doubt as to whether it was the peritoneum or a portion of the cyst. After the most careful dissection, in which he had the able assistance of Messrs. Porter and O'Grady, he found it impossible to decide the question. A quantity of the jelly-like substance came out of the incision that had been made. It looked like fat, and his first impression was that it was the fat that was found lying between the peritoneum and the fascia transversalis. He made a second incision nearer the umbilicus, and then found that he could pass a probe through the opening where the cyst had originally ruptured at the umbilicus, and bring it out at the first incision. There was no longer a doubt that he had opened the cyst. The two incisions were now connected with one another, and the opening was extended up above the umbilicus, but still no point could be found where the cyst and peritoneum were not closely united. He now passed his hand into the tumour and broke down the septa running through it, and removed a large quantity of the jelly-like substance, after which he proceeded to peel off the fibrous layer of the cyst from the peritoneum, enucleating it somewhat after the method of Dr. Miner. In doing this, portions of the cyst wall were left where the separation could not be effected. The adhesions were as close posteriorly as anteriorly, but at length the pedicle was reached. A ligature was applied, the tumour removed, and the pedicle dropped into the abdomen. There was no bleeding, the cavity was sponged out as carefully as possible,

and the incisions closed, leaving the ligatures hanging out. The tumour was recognized as a specimen of the alveolar cancer of Cruveilhier. It had been examined microscopically by Dr. R. J. Harvey, who had written the following report as to its nature:—"The tumour you left for me is, I consider, an example of colloid carcinoma. The opaque portions present a delicate carcinomatous stroma, in the alveoli of which are numerous somewhat small cells embedded in colloid material; both cells and stroma are undergoing rapid degeneration, probably fatty, and hence, doubtless, the white appearance. In the most transparent part little or no structure is discernible, and the whole seems to have completely undergone colloid degeneration. It is an interesting tumour; I never saw one exactly like it before." In reviewing the case it seems they were justified in the conclusions that it was a multilocular tumour, with very extensive adhesions anteriorly and superiorly, and to the uterus. The diagnosis was incomplete, inasmuch as the posterior adhesions were not recognized, or the cancerous nature of the tumour. It is unfortunate that we have no means of knowing the existence or extent of posterior adhesions till they are met with in the course of the operation, especially as (though anterior adhesions do not to a great degree increase the risk of the operation) those situated posteriorly, or in the pelvis, lessen very materially the chance of recovery. The rapid growth of the tumour after the 25th November might have raised a suspicion as to its malignant nature, but, on the other hand, its growth up to this date had been slow, and the woman always maintained that her health was unimpaired, except for the tumour. The coagulation of the contents of the tumour which occurred after the rupture of the cyst at the umbilicus was rather a rare occurrence, but it sometimes occurs after repeated tapplings. The fluid drawn by the tapping (and that which first escaped after the rupture) was thin and serous, and on examination with the microscope it was found to contain nothing but degenerated blood, corpuscles, and epithelium. Fluctuation was at first very distinct; but as soon as coagulation occurred, the fluctuation was converted into a boggy elastic feel, and the umbilical protrusion became irreducible. It was always felt that the case was a most unfavourable one for operation, but one in which, if the patient demanded the removal of the tumour, knowing the risk, she should not be refused. This was very fully explained to the woman, her mother, and husband, and she begged to have the operation done. Under these circumstances he thought it his duty to comply with her request. When the character of the tumour, and the nature and full extent of the adhesions were discovered, they debated anxiously and seriously, but quickly, whether they should proceed or not, and they thought that if they proceeded with the removal it would not lessen her chance of recovery. It was almost hoping against hope, but at the same time they knew that some of the most remarkable recoveries had occurred in apparently hopeless cases.

In the present instance, however, the woman did not recover. She lived for eighteen hours, and then sank from collapse.—*December 20, 1873.*

*Cerebro-spinal Fever.*—DR. GRIMSHAW exhibited the brain and spinal cord of a patient who died in Steevens' Hospital of cerebro-spinal meningitis. The patient was a man aged twenty-three, a railway porter at the King's-bridge Station of the Great Southern and Western Railway. On the 13th of December he was perfectly well, went to his work as usual, and then went to Grafton-street (a distance of about a mile and a half), where he was seized with a sudden pain in his head, vomited green matter, and had a shivering fit. He with difficulty returned home, and went to bed. He was delirious on the night of the 14th, and was admitted into Steevens' Hospital on the 16th of December, when, on being put to bed, he lay on his side, with his head drawn back, his eyes wide open, and turned upward. He was delirious when left to himself, but talked almost collectedly when spoken sharply to. There was a large patch of herpes extending from the left angle of his mouth along his left cheek and side of chin. The vesicles were closely set, small, and to a great extent confluent. Another band of more scattered vesicles existed over the left clavicle. Temperature,  $101^{\circ}8$ ; pulse, 90 and soft; very restless; respiration normal; pupils normal; little, if any, hyperæsthesia; no black spots; no rigidity of limbs; bowels confined for some days.

Next day symptoms much the same, with constant hiccough. The symptoms continued to increase, but no new ones were added, except great congestion of the eyes, especially of the sclerotic vessels; the pupils became dilated, but contracted before death.

Immediately before death (which took place at noon on the 18th) an attack of opisthotonos occurred, the convulsion being extreme. Mr. C. G. Maturin, the clinical pupil who kept these notes, was present at the time, and observed the convulsion.

A *post-mortem* examination was made by Dr. Bookey twenty hours after death, with the following results:—Rigor mortis well marked; the whole of the back of the body dark and livid; on the shoulder (not merely on the depending parts) small, dark, raised spots, feeling like grains of shot, were observed. These spots, similar to those so often observed in many cases of this disease during life, may have appeared immediately pending death, but could scarcely have escaped Mr. Maturin's notice. On making incisions for the removal of the spinal cord, the usual large quantities of dark tarry blood flowed from the vessels.

The cord was found much congested, softened in one or two places, especially at the middle, dorsal, and upper cervical portion; patches of lymph deposit were visible at many points along the arachnoid of the cord, but these were not so well marked as often found in such cases.



On opening the head the sinuses were found distended with dark blood, which flowed out in large quantities. The arachnoid covering the cerebral hemispheres was congested, and quickly assumed the intense ros  l tint familiar to those who have examined these cases. The base of the brain presented an appearance somewhat different from those seen in the majority of the cases which had been brought under the notice of the Society. Although there were slight opacities of the membranes at many points, yet nearly the whole of the lymph deposit was limited to a comparatively circumscribed locality; it extended from the optic commissure in front to the middle of the medulla oblongata behind, occupying the middle third of the base of the brain from side to side. The deposit completely hid the front and the origin of nearly all the cranial nerves, the nerve and large vessels at the base of the brain were completely imbedded to this mass of greenish-yellow lymph, which at its thickest part measured three-eighths of an inch.

Dr. Grimshaw remarked that the occurrence of this severe case, together with some milder ones of cerebro-spinal meningitis, seemed rather to portend an epidemic in Dublin.—*December 20, 1873.*

*Ulceration of the Cartilages of the Knee-joint.*—PROFESSOR W. STOKES exhibited a specimen of ulceration of the cartilages of the knee-joint. It was taken from a lady aged twenty-seven, whose thigh he had recently amputated on account of this disease. She was a person of very delicate habit, was married, and the mother of one child, which, he might mention, she had nursed for upwards of eighteen months, and was consequently in a very debilitated state of health. She returned from America, where she resided with her husband, for the purpose of attending her mother, who was in a dying state. She remained in close attendance on that lady for several months. On her death she started on her return to the United States, coming to Ireland on her way to visit some friends here. She arrived here in July last, got a very severe cold and wetting, and observed a few days afterwards that the right knee was considerably swelled. She paid very little attention to it for a few days. She then summoned a skilful practitioner, Dr. Cahill, who recognized it as acute synovitis. He prescribed for her; but the patient, being a very self-willed, irritable, obstinate person, the advice given to her was not by any means strictly followed. He (Professor Stokes) saw the patient shortly after this in consultation, and substantially endorsed all that had been recommended, but to no effect. Nothing which he proposed would she follow. Other surgeons saw her, but she treated them all in the same way. She would try the remedies for a few days, and then throw them aside. As a necessary consequence of this unwise conduct on her part, the case went on from bad to worse, and six weeks after his first visit he was again brought to see her. Then, indeed, the case had changed

considerably. It was at first a case of pure synovial inflammation, but now it had all the symptoms and signs of ulceration of the cartilages. There was the most exquisite tenderness in the affected part. She screamed on the slightest motion being made in the room. The leg was drawn up so that the heel touched the buttock, and any attempt to extend it was out of the question. Morphia, given either subcutaneously or by mouth, had not the slightest effect in diminishing the starting pains in the joint. The muscles of the thigh, as well as of the leg, were much wasted. She sweated at night, and had a brilliant hectic flush on her face. All idea of saving the joint or of performing resection was out of the question. He accordingly recommended amputation of the thigh, and this he performed, adopting the rectangular method. The case had made a most excellent recovery. On examining the joint there was found to be very little effusion into it. There was a small quantity of sero-sanguineous fluid and a large number of small clots of blood in the interior of the joint. The cartilages were extensively diseased, the crucial ligaments gone, and over the external condyle there was extensive ulceration, the cartilage being completely destroyed towards the posterior aspect of the bone. The same condition was commencing on the inner condyle. The cartilage was quite thin in some places, and doubtless there would have been the same condition as that on the external condyle if the operation had been deferred a short time longer. The bone was much softened and eroded, the result of sub-chondral osteitis. On that portion of the head of the tibia on which the external condyle rested, the cartilage was completely gone; nothing remained but a ring almost completely detached, and the bone itself was quite soft. There was a lesser condition of ulceration on the other side. The patella presented a remarkable condition. The cartilage was completely gone, and the bone extensively eroded. He considered this was an excellent example of a form of disease not usually seen, and was of considerable clinical interest because of the great rapidity of the disease. The lady was first attacked at the end of July, and in the course of eight weeks this enormous destruction of the joint had occurred.—*December 13, 1873.*

*Extreme amount of Intestinal Ulceration in Enteric Fever, with Constipation.*—DR. A. W. FOOT exhibited the intestines of a young man, aged twenty-three, who had died from enteric fever on the twenty-fifth day of his illness. The cause of death seemed to have been hypostatic pneumonia of the whole of the posterior portions of the right lung. The physical signs of this complication were observed early in his illness, although there was a complete absence of pain in the chest, cough, expectoration, or of any complaint with reference to the side. For twelve days after admission he had no motion from the

bowels of any kind; he had castor-oil in doses of four, six, and eight drachms, on three successive days, without effect; on the fourth a turpentine enema was used, which brought away a large amount of formed fæces; on two subsequent days there was a single motion each day, but not of the kind considered characteristic of enteric fever. There was but occasional and very slight tenderness of the belly. The eruption was represented by three or four spots, which, as there was a good deal of acne vulgaris on the back in various stages, were not in themselves at all diagnostic. He was a full, fat, heavy "lump of a lad." The extreme prostration of mind and body was very obvious; his intellectual qualifications seemed to be inherently obtuse, and his habitual listlessness had increased to an apathetic, monosyllabic state; he was unusually long in replying to a question of the simplest kind. His relatives said he had always been very heavy and stupid in his manner, and dull in his mind, especially when at all sick. The bladder was weakened soon after admission, he wetted his bed, and required for some time the use of the catheter. His illness was apparently traceable to cold caught on an outside coach; and, when admitted to hospital, the symptoms and physical signs of extreme congestion of both lungs, but especially of the right, were present. He was bled from the right arm to eight ounces, and wet-cupped over the right back on the same day, with relief to his dyspnœa. Subsequently, on two occasions, he was leeches behind the ears. From the seventeenth day there was more or less locomotive delirium at night. On the twenty-fifth day he died, at 9 p.m. On the morning of this day his temperature was 104° F., and he was muttering in a semi-unconscious state.

At the autopsy, fifteen hours after death, the cadaveric rigor was very marked. The surfaces of the hemispheres of the brain were found very perceptibly softened, and numerous unusual patches of chronic arachnitis parallel to the superior longitudinal sinus existed. There was no evidence of peritonitis. The last six inches of the ileum presented forty-seven ulcers, varying in size from a hempseed to aggregated oval patches one and a half inches in length; there was in addition a confluent mass of ulceration surrounding the superior margin of the ileo-cæcal valve, and numerous pustular ulcers in the cæcum. The large intestine contained a quantity of solid fæcal matter, the ileum much ochreous fluid. The ulcers were of the elevated, rugged, tuberculated kind, with adherent ochreous sloughs. The large and firm spleen weighed 15½ ozs., and was accompanied by two supplementary spleens, of the size of large cherries. The whole of the posterior half of the right lung was solid, sank in water, and was of a dark reddish-brown colour; the back of the left lung was in a less advanced degree of a similar condition.—*December 20, 1873.*

*Stricture of the Urethra.*—DR. BENNETT said the viscera which he

exhibited had been taken from the body of a man who died on the previous night in Sir Patrick Dun's Hospital. His history was the usual one of stricture of the urethra in many respects. He had been abroad some three years ago in the Mauritius, and suffered then from severe attacks of the intermittent fever of the country, and also from dysentery. At the same time he was suffering from stricture, which increased on his return to Dublin. Shortly after his return he was admitted to Baggot-street Hospital, and was there under the care of Mr. Tufnell. Dr. Bennett did not know what form of treatment was adopted for the stricture, but a train of symptoms was set up, according to the patient's account, similar to those which occurred in his last illness—this he considered to be a return of the ague which he had suffered from while abroad. The only evidence found in the viscera bearing on the ague complication was the appearance of the spleen. There was no chronic disease of the heart, or of the lungs, except a slight emphysema. There were some pathological appearances in the brain, evidently recent, but the only substantial change (except, of course, in the urinary organs) was found in the spleen, which was much larger than natural. The liver was perfectly healthy. For some time after admission to Sir Patrick Dun's Hospital, great difficulty was found in getting an instrument into the bladder. Dr. Bennett made two or three ineffectual attempts with a very fine bougie. He finally reached the bladder with a Hutton's railway catheter, size, No. 2. That was tied in and retained for twenty-four hours. The man was suffering, on admission, the most extreme distress, from the constant dribbling of urine, but in other respects appeared to be quite healthy, and a well nourished strong man. There was considerable distress occasioned by the presence of the instrument in the bladder, and it was removed after twenty-four hours. Next day an attempt was made to re-introduce an instrument of the size passed before, but only No. 1 could be got through the stricture. It was tied in and retained for some time, and the same process was repeated several times without effecting any dilatation of the stricture. It was never possible to introduce a No. 3 catheter. Some more active treatment was clearly desirable—the sole difficulty in the matter being the getting a No. 2 through the stricture. In order to introduce a Holt's dilator, it would be necessary to introduce a No. 2. A No. 1 tangle tent bougie was introduced and retained for an hour. This was passed about a quarter of an inch beyond the stricture. In an hour and a half it was removed, in consequence of the distress it occasioned, and the desire the man had to pass water. Some force, but not much, had to be used in its removal. It came away with a steady strain, without hæmorrhage, and the pain of withdrawal lasted but a minute or two. The tangle tent which he used was No. 1 at its point, and it had dilated to No. 6 when it was withdrawn. The bougie showed, by its constriction at one part, the pressure of the stricture. It was removed



at 12 30. At 3 p.m. the patient complained of a great desire to make water, and also of a dread of attempting to make it in consequence of the soreness of the passage. He was induced to make the attempt, and passed some water and a small quantity of blood with it. At half-past four he was observed to be sitting over the fire, shivering; at six o'clock he was violently delirious, and the delirium reached such a height that he knocked the ward furniture about, and required so much force to restrain him, that Dr. Bennett was sent for at seven o'clock. He was then quite unconscious, knew nothing of what was passing around him, and falling into a mild state of perspiration, but had got quiet. While Dr. Bennett remained in the ward he gradually recovered consciousness, so as to answer questions. From that time his symptoms progressed rapidly. That night he slept, but suffered greatly from headache in the night and morning. The temperature was  $103^{\circ}$ , and the pulse 140. He had no pain on the 3rd of January, but had two rigors of the same character, but not quite so violent. The temperature was then  $104^{\circ}$ . On the 4th and 5th he had three or four rigors, and the temperature was  $105^{\circ}$ . His temperature remained at  $105^{\circ}$  until he died on the 9th, and his rigors became more frequent, occurring every second or third hour. Dr. Bennett never saw him during the last three or four days of his life that he was not in a rigor. If this were a purely urinary fever, it was certainly more violent than usual. When in *articulo mortis* on Friday morning (the day previous to the meeting of the Society), the conjunctiva of one eye was extensively ecchymosed. He died with a brown tongue, and in a state of unconsciousness. When the body was examined, the chest was found free from any sign of disease, except the merest trace of recent lymph in one pleura. The lungs were slightly emphysematous, but there was no consolidation or bronchitis. There was, in fact, no intra-thoracic disease, except what might have been induced by the violence of the respiratory efforts during the last two or three days. The pericardium and heart were healthy. The viscera of the abdomen were healthy, and covered with fat, not excessively, but to a considerable amount. The kidneys were the first of the urinary organs removed, and both were found to be exceedingly soft. They were almost the colour of ordinary fat, and the tissue was evidently in a state of extreme fatty degeneration. On laying open the hilus of the kidney from without, there was found lying in it a purulent urinous fluid. The whole of the urinary tract was inflamed and covered with a mixture of pus and urine—the ordinary phenomena of acute suppuration of the hilus.

On separating the bladder from the rectum, and looking into the rectovesical cul-de-sac, he found projecting backwards from the base of the bladder the wall of a considerable abscess. It was filled with a deep grumous pus mixed with shreds of recent lymph. The whole of the surface was covered on the inside with lymph, and the odour of it was

extremely fœtid and slightly urinous. When the bladder was laid open, they found the usual hypertrophy of the wall met with in cases of chronic stricture, the muscle being rendered as thick as the wall of the left ventricle of the heart.

The interior of the viscus was of a deep murillo cherry colour, almost black. Passing out of the bladder, this colour ceased. In the prostatic portion of the urethra, the abscess seen in the recto-vesical pouch had effected an opening. Deep in the prostatic sinus there was a small irregular opening effected by ulceration, and it was clear that some pus escaped during the last days of life. He thought the opening of this abscess was so placed, and of such a character, that it was not caused in any way by a false passage. At the junction of the membranous and bulbous portions of the urethra, about a quarter of an inch in front of the membranous portion, there was a constriction of the urethra, which was caused by a deposit in the sub-mucous tissue, and on the surface of the mucous membrane, of a material of gristly appearance and hardness. This extended entirely round the urethra, and occupied nearly an inch of its length. Along the floor of the urethra an irregular rent divided the stricture through its whole length, and it was evident that the rupture had been caused by the dilatation of the bougie. No false passage existed at the seat of stricture, nor did the abscess seem in any way related to it; its opening was separated from the stricture by nearly the entire length of the prostatic part of the urethra.—*January 10, 1874.*

*Malignant Tumour of the Uterus Removed by the Ecraseur.*—DR. LOMBE ARTHILL said the specimen he wished to exhibit was of considerable interest in several respects. It was especially interesting as bearing on the possibility of cure, or at least of good results following from having recourse to surgical operation in some cases of malignant disease of the uterus. The specimen before them was the entire of the vaginal portion of the uterus, which he removed on the previous Thursday from a patient in the Adelaide Hospital. She was admitted two or three days previously, suffering from what was supposed to be incontinence of urine. There was not the slightest idea that any uterine disease existed. She had no pain in either the back, abdomen, or loins. She merely felt weak, and suffered from a constant watery discharge, saturating her linen by day and night with what, on a superficial examination, seemed to be urine. She was placed under the care of his colleague, Dr. Head, but he soon came to the conclusion that the bladder was not in fault, and placed her under his (Dr. Arthill's) care. He found, on examination, a large mass of malignant disease springing from the lower segment of the uterus, but on passing his finger above this mass, he came on what appeared to be healthy tissue. He, therefore, determined at once to remove the whole of the cervix. The enclosing of it with the wire of an ecraseur was not

a matter of difficulty, because the mass did not extend high up, but the tissue to be cut through was so dense that he broke the wire twice, and finally bent the ecraseur, and it was not until he obtained the strongest pianoforte wire and another ecraseur that he was able to sever this. He believed, however, that he had succeeded in removing the whole of the diseased portion. There was considerable hæmorrhage, which was controlled by the application of the perchloride of iron. The points of special interest in the case were the total absence of symptoms, the erroneous diagnosis, and the limited extent, as it appeared to him, of the disease. The patient was under chloroform during the operation, which lasted more than half an hour. She soon came from under the influence of the anæsthetic, and was doing well. She was a married woman, forty years of age, had borne several children, and menstruated normally up to the present time, the constant watery discharge being the only symptom of which she complained.—*January 10, 1874.*

*Hepatic Carcinoma.*—DR. A. W. FOOT exhibited some of the viscera removed that morning from a man aged fifty-five. The liver, which weighed 7 lbs. 9 ozs. presented carcinoma. The primary disease was considered to have been a large, soft, fungating, condylomatous-looking ulcer, at and surrounding the œsophageal foramen of the stomach. The spleen, which weighed 11 ozs., was uninfected, shrivelled, and wrinkled, of a pale slate colour. The kidneys weighed together 11 ozs.—the left, 6; the right, 5 ozs. The lungs presented many evidences of previous disease, in addition to which there were very numerous firm nodules disseminated through them, shotty, or like marbles, to the feel, thinly covered with the blackish-green lung tissue, but of a cream colour when cut down on and incised. Both lungs were adherent at many points to the sides of the chest, and the base of the left one was intimately adherent to the upper surface of the diaphragm; the right apex was much puckered, and scored irregularly with cicatricial depressions. Alternating with these scars were emphysematous bullæ. There was also marked emphysema of the anterior lobe of the left lung. The posterior and inferior portions of both lungs were deep bluish black, leathery, flabby, and non-crepitant. The hard nodules in the pulmonary tissue were much more evident to the hand than to the eye. There was no effusion in the chest.

The carcinomatous nodules apparent on each surface of the liver were of a creamy white, varying in size from that of half-a-crown to that of a haw. Many presented the dish like summit depression called the carcinoma navel, due to the peritoneal aspect, or summit of the nodule, falling in, and becoming concave, owing to the resorption of the fatty detritus produced by the degeneration of the oldest and most centrally situated cells of each nodule.

Over the surface of the liver there were evidences of both chronic and

recent peritonitis—of the former in the various degrees of milky thickening of the peritoneal coat, of the latter in a thin sheeting of exudation fibrine over the interior surface of the right lobe in particular.

The gall-bladder, contrary to what is usually observed in cases of hepatic carcinoma, did not contain any calculi; there was no marked impediment to the outlet of bile; the patient had not while under observation any tint of icterus, even in the conjunctivæ; nor did the tissues of the corpse in any way indicate any biliary stain.

There were two clinical features which led Dr. Foot to regard the ulceration at the cardiac orifice of the stomach as the cause of the hepatic disease—one was that there had been persistent pain in the left shoulder for about eight months before his admission, with several symptoms of dyspepsia; the other was that the ascites had only occurred a month before admission, from which he would infer that the dissemination of the disease through the liver had been comparatively recent, and subsequent to the ulceration in the stomach.

It is established as a pathological fact that more than three-fourths of all hepatic cancers are metastatic, and of the metastatic again that two-thirds are metastases, after primary disease in the region of the portal vein.

The heart was unusually small for the size of the man, weighing 7 ozs. The lining membrane of the arteries, the aorta and pulmonary, was stained of a cherry red, indicating a premature decomposition of the blood corpuscles. The examination of the body was made twenty-five hours after death, and the weather was cold, so that the endo-arterial discoloration was not the result of ordinary putrefaction. By some the atrophy of the heart in carcinoma was held to be greater than that observed in phthisis. The cardiac atrophy was general, and there was a strictly relative reduction in the dimensions, cavities, and walls.

The patient who formed the subject of this communication had been a week in hospital; he sought admission on account of ascites. Sudden pressure over the epigastrium displaced the fluid, and the hand came down with a shock upon a rocky tumour, one of the most prominent tubera in the left lobe of the liver. The hepatic dulness commenced one inch below the nipple. Increasing urgency of dyspnœa led to the performance of paracentesis, when he was almost moribund; he survived the operation but a short time. The fluid removed measured 174 oz., and was thin and clear. The small quantity which remained in the peritoneal cavity of the corpse was turbid and thicker, and exhibited a sedimentary deposit of a purulent nature in the depths of the pelvis. Dr. Foot considered it largely due to a peritonitis with sero-purulent effusion, the result of the irritation, or possibly the rupture of some of the numerous tubera. Part of the ascites must have been due to increase of portal tension, and the œdema of the lower limbs and genitals was



explicable by pressure on the inferior cava of the large and heavy liver. The liver measured 12 inches from right to left, and its greatest vertical dimension (from thick to thin border) was  $8\frac{1}{2}$  in.; the right lobe was, in its transverse measurement,  $7\frac{1}{2}$  inches, in its vertical  $8\frac{1}{2}$  in.; the left, in its transverse,  $4\frac{1}{2}$  inches, in its vertical  $6\frac{1}{2}$  in. The tubera were more numerous in the left than in the right lobe. There were no enlarged glands in the portal fissure. The large boss most perceptible during life was in the left lobe, to the left of the broad ligament, and just above the notch of the inferior margin of the longitudinal fissure; this tuberosity was the projecting portion of a mass which on section measured 3 in. by  $4\frac{1}{2}$  in.—*January 10, 1874.*

*Congenital Excess of Ureters.*—DR. WALTER SMITH exhibited a kidney possessing a double ureter, which had been taken from a body in the dissecting room of Trinity College. The point of interest in the case was that the duct was double throughout the whole extent, from the kidney to the bladder. It was the right kidney which presented this abnormality, that on the left side being normal. At first it seemed as if the two tubes were joined together at some distance from the bladder, but by careful scraping, the two ducts could be separated down to their entrance into the bladder. The occurrence of a double ureter was sufficiently rare to make the case worth exhibiting. He could find only two cases recorded in the proceedings of the London Pathological Society. In the first case there were no particulars given, and in the next the union of the two tubes took place at a distance of about four and a half inches from the kidney.—*January 10, 1874.*

## TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

President—JOHN MOORE, M.D.

Hon. Secretary—H. S. PURDON, M.D.

*December, 1873.*

*Excision of the Elbow.*—The PRESIDENT introduced a patient whose elbow he excised on the 9th July last. The patient, a fine healthy-looking lad, aged thirteen years, while following his occupation of attending to a machine in a paper manufactory, had his left elbow caught between a strong projecting pin on a revolving roller and the frame of the machine, whereby the humerus was broken just above the condyles, and the condyles themselves split and comminuted, the external one being partially driven out through a wound about two inches long, situated just over it. It was evidently a case for operative interference, and excision of the joint was decided on. The patient was placed under the influence of chloroform, and an incision made to the outer side and parallel with the ulnar nerve, which was carefully separated with the surrounding tissues from the internal condyle, and drawn, by means of a retractor, inwards and out of harm's way, another incision was now carried across the joint so as to connect the wound made by the accident with the first incision, and thus render the operation the same as that by the old H-shaped incisions. The articulating end of the ulna was cleared and sawn off, leaving the head of the radius intact; the fractured condyles and comminuted portions of bone were easily separated, and the fractured end of the humerus trimmed with the cutting forceps. The wound having been cleared, the edges were drawn together by wire sutures, and the arm placed on an angular splint. The wound healed kindly, and there was very little constitutional disturbance. He was discharged on the 13th September, with the wound completely healed, with power of flexion to a right angle, and with perfect use of hand and arm. He is now following his former occupation, with but little inconvenience from the injury he suffered.

*Case of Labour Complicated with Abnormal Presentation.*—The PRESIDENT said:—On Saturday, the 1st November last, I was consulted by Mrs. W. with regard to the propriety of the induction of premature labour. She stated that she was in the eighth month of pregnancy with her fourth child, and gave me the following history of her previous confinements:—Her first was premature, and the child was dead some time; this labour was, notwithstanding, terminated by the aid of the forceps. Her second

confinement was at full term, and she had to be delivered by craniotomy. Her third labour was "induced" at the eighth month, but even this did not succeed in saving her child, as her delivery had to be accomplished by the forceps, and the child only gave a gasp and expired. Under such circumstances I arranged to visit her on the following Monday and make a careful examination of the pelvis and inform her what I should advise her to do. She was unwilling to have labour induced, as she suffered more from it than in her previous confinements; she was of low stature and of slender build. Before the hour for my appointed visit on the Monday, I received a message to say that she was ill, and requesting me to call at once. On my arrival I found that labour pains had commenced, so the first problem was solved most satisfactorily, as on examination I found the cavity of the pelvis so contracted, and the promontory of the sacrum so projecting, that the normal space was diminished by more than an inch. I concluded that she was nearly correct with regard to her time, as a considerable portion of the cervix uteri was still undeveloped. The head was found presenting. At one o'clock, three hours after my first visit, some progress had been made, the pains were recurring at regular intervals, and the os was dilated to the size of a crown piece, the membranes still unruptured. The uterus and child still kept high and did not descend into the pelvis. At four o'clock, on calling, I found the patient getting into bed from the night chair on which the membranes had ruptured, and, on making an examination, I found the cord still pulsating lying in the vagina, with a hand and arm projecting beyond the head. As commencing labour had solved the first problem which the case presented—namely, whether it was one for "induction of premature labour," or whether she should be allowed to go on to the full period of pregnancy, so the second problem was now solved—viz., what method of delivery should be selected. At this stage of the labour, when it was not improbable that craniotomy might have to be resorted to, I requested the assistance of Dr. David Johnston, who kindly aided me. As the uterine contractions were fortunately not violent, I found but little difficulty in the introduction of the hand into the uterus, but considerable difficulty in finding a foot, as the child seemed twisted in a most peculiar manner; having succeeded, however, and bringing one down, the head did not revolve, and there was now in the vagina a hand, a foot, the cord, and the head at the brim. As the foot seemed most inclined to return to the cavity of the uterus, I placed a loop of tape around the ankle which kept it under control. The absence of strong uterine contractions was very favourable for remedying this mal-position, which a little manipulation and perseverance accomplished, and the breech and body were at length brought down and the arms were relieved. The pulsation in the cord was becoming very feeble, though it could still be felt, but all our efforts to get away the head, by any manœuvring or traction compatible with

the life of the child, failed. I succeeded, however, in extracting it without reducing it, but only to see it make one or two feeble efforts at respiration and then expire. The child was a male, and as large as an average one at the full period.

DR. J. MACCORMAC asked if the deformity was in the sacrum.

DR. CHARLES gave some interesting facts regarding the different measurements of the pelvis.

The PRESIDENT, in reply, said that the deformity was occasioned by the sacrum, and he thought that the best pelvimeter was the finger.

DR. H. S. PURDON remarked that Dr. Murphy, of London, in his work on Midwifery, advised the student not to trust to pelvimeters, however ingeniously contrived, and held that time and a close attention to the symptoms which present themselves decide upon the line of treatment to be taken.

*Amputation and use of Esmarch's Means of Restraining Hæmorrhage.*—The PRESIDENT read notes of the following case:—M. P., aged forty-five, a stout strong man, was engaged on a ladder at his employment as a carpenter, when it slipped, and he fell from a height of more than twenty feet, fracturing the left femur immediately above the knee-joint, separating and splintering the condyles with complete dislocation of the fragments, and a large wound opening into the joint. The case was one demanding immediate amputation, to which the patient readily consented. It was determined to try Esmarch's method of restraining hæmorrhage and an elastic roller was carried from the toes to the upper portion of the thigh, a pad of lint having been previously placed over the wound. Chloroform was then administered, and when the patient was fully under its influence, a band of vulcanized India rubber was carried round the limb, and fastened by hooks attached to each end. The roller was then unwound, and the limb amputated as close to the injured parts as the state of the wound would admit, the bone being sawn through about the junction of the lower with the middle third. There was scarcely a trace of blood to be seen during the formation of the flaps, which were of skin, or the division of the soft parts and separation of the limb. A ligature was placed upon the femoral and the constricting band removed. Two small vessels spouted and required ligature, and there was some regurgitation from the femoral vein, which also had to be tied. Not more than two ounces of blood were lost during the operation. The wound healed by primary adhesion, except at the angles where the ligature lay; the last separated on the fifteenth day; the completion of healing was effected in three weeks, and the man discharged at the end of four weeks from the receipt of injury.



In this case, though the man was stout, plethoric, and robust, with a thick limb, there was no symptom of local congestion or other evil resulting from pressing back into the circulation the blood contained in his limb, and he had lost none from the accident. A tendency for the edges of the wound to slough has sometimes been supposed to result from the application of the elastic bandage, but in this case the wound healed most kindly, and his progress to recovery was uninterrupted.

*Dislocation of Hip-joint Reduced by Flexion and Rotation.*—The PRESIDENT stated that on the evening of the 21st November, 1873, he was requested by Dr. Newett to see with him a patient, whose left hip had been dislocated by the upsetting of a cart in which he had been riding home from Belfast. The usual symptoms of that accident were well marked. Reduction was effected by grasping the ankle firmly and flexing the leg upon the thigh, and then carrying the thigh upwards towards the abdomen, and on rotating the limb outwards it slipped into its place with the greatest ease. The simplicity and facility of this plan of reducing these hitherto troublesome dislocations, and its advantage in this case were so striking as to induce him to bring it under the notice of the members of the Society.

*Noma Pudendi.*—DR. FAGAN introduced a patient, who was under his care at the Belfast Children's Hospital, suffering from the above. The disease had assumed a malignant form, and was arrested only by the free application of the strong nitric acid. He remarked that, in his experience of such cases, the only effectual and safe treatment was the immediate application of some powerful caustic, with tonics and stimulants internally. The dressings used were of a stimulating character. The child above exhibited was nearly well, but a drawing represented the disease on admission at its worst stage.

January, 1874.

*Lupus Erythematosus.*—DR. H. S. PURDON made the following observations on this comparatively rare disease:—I have had two cases of *lupus erythematosus* recently under treatment, and rather than describe them separately I will take this opportunity of saying a few words on the disease. Both patients were females—one, Mrs. T., aged fifty. In her case the disease occurred on the chin, commencing ten years ago as a small red spot, which became covered by a tenacious scab, nearly impossible to remove; it slowly spread, and when seen by me was about the size of a two shilling piece, mottled at one side—this white appearance being due to the growth of new white fibrous tissue. Her family history was bad, phthisis having carried off several of her relations. The treatment adopted was frictions with juniper tar soap, painting the

part afterwards with a calamine lotion. Subsequently I prescribed a weak iodide of sulphur ointment, and tonics internally. The disease, after five months' treatment, is now cured. My other case, which I attended at the same time, occurred in the person of Mary P., aged twenty, a mill worker. Both cheeks were attacked, as was also a part of the left ear. The skin was red; orifice of gland ducts plainly visible, and filled with "horny exuviae," bearing out the view of Hebra that the disease is, in the first instance, an affection of the sebaceous glands, or a *seborrhœa congestiva*, as he calls it. In this case I ordered a preparation containing oil of cade, and a mixture of iron, wine, and Fowler's solution. The disease is slowly improving. When the disease occurs near the scalp permanent baldness is produced. In both these cases the disease will terminate in a white depressed cicatrix. Bielt called this disease *erythema centrifugum*; but, as Erasmus Wilson observes, "at a first glance the patch appears trifling; its stationary habit and resistance of treatment excite suspicion that it is something more than common erythema, and its disposition to occasion atrophy of the skin proves it to be more serious in its nature, and when it fortunately disappears spontaneously, the white cicatrix indicating the removal of the papillary layer of the derma, or, on the scalp, the destruction of the hair follicles, declares its relation to lupus." I recall to mind the case of a young lady who was under my care for this disease some four years ago, and it made sad havoc of her face; both sides were involved, and the hair all gone at both temporal regions. The disease under notice often attacks the fingers and toes, and is then confounded with chilblains; it is sometimes associated with lupus non-exedens. In lupus erythematosus, as well as in lupus non-exedens, the capillaries going to a lupus papule or patch are twisted round the clubby kind of cells. In the first mentioned variety it is the true skin that is chiefly involved; whilst in the other, or non-exedens, it is the connective tissue of the skin that is affected. As pointed out by Volkman in lupus erythematosus, the skin is often at first covered by a leafy crust, generally symmetrical. Where the nose and both cheeks are affected it has been compared to a butterfly with wing expanded. This crust adheres tenaciously to the reddened skin, and has often an unctuous feel. If this crust is removed the skin underneath is found red and apparently purely papillary, the latter appearance, by aid of a lens, is seen to be due to a number of minute orifices closely set together, which are the openings of sebaceous glands much dilated, and the crust is caused by their secretion. Volkman holds that in addition to the seborrhœa there is infiltration of lymphoid elements. I may say that in my recent case I was able to point out all these characters to the gentlemen attending my clinique at the skin hospital. The treatment has already been touched on; however, it has been suggested to remove the crusts by lint soaked in oil, and then washing the diseased part with

a good lather of soft soap. If this fails strong liquor ammoniæ or liquor potassæ is applied, or iodine, iodide of potassium and glycerine pencilled on the part; others prefer Rochard's ointment, composed of calomel, iodine, and conium ointment. Carbolic acid has also been tried. As in the other varieties of lupus, internal remedies are useless, it is to local means we are to trust.

DR. M'CREA considered that constitutional treatment was most important in all forms of lupus, and held that the disease was generally connected with scrofula.

DR. WALTON BROWNE mentioned a curious skin affection occurring on the fingers and toes, chiefly the latter, in the boys who are sent on board the "Gibraltar" training ship in Belfast Lough. The skin becomes red, vesication then occurs, and an indolent ulcer ensues. He thought the disease allied to erythema gangrenosum; that it was not chilblains was proved by the fact that it occurred during summer as well as winter. He had found resin ointment and good diet the best plan of treatment. When the toes were attacked the ankles usually became œdematous.

DR. H. S. PURDON replied that he thought the local treatment of lupus, especially by what is called "heroic" means, the best plan, and had no faith in a "soothing" plan of treatment, except at first, when symptoms of irritation were present. He noticed that erysipelas had cured lupus vulgaris of the face. Dr. Purdon referred to Dr. Theodore Veiel's treatment of lupus. The best results being obtained by chloride of zinc in solution in alcohol and applied to the affected parts, which had in the first instance been blistered. This treatment is very painful, and not likely to be submitted to by Irish patients.

*Gastric Ulcer.*—DR. CHARLES exhibited a very interesting specimen of ulcer of the stomach. The patient, aged fifty, had been in the Union Hospital. The stomach was contracted to about one-third of its original size, and all its coats greatly thickened. A small ulcer was also apparent. He had examined microscopically for cancer, but the result was negative. The heart was also atrophied. Dr. M'Crea asked if there was any history of syphilis, or if he had examined for amyloid deposit.

DR. FAGAN thought that there was some peculiar tendency to atrophy generally, as other organs were said to be smaller than normal.

DR. M'KEOWN held that excessive tea drinking, as well as abuse of alcoholic stimulants, was injurious; indeed he thought the former led to disease of the mucous coat of the stomach much more frequently than the latter.

DR. H. S. PURDON stated that the prolonged abuse of alcoholic drinks gave rise to a catarrhal state of the stomach in the first

instance, followed by a new growth of connective tissue, which finally pressed on and obliterated the follicles, causing an inveterate form of dyspepsia. He also stated that the modes of death from gastric ulcer were generally held to be three in number—by perforation, by opening into a blood vessel, and thus inducing hæmorrhage, and by debility and exhaustion consequent upon the vomiting and non-absorption of food. He held that in cases of gastric ulcer sugar was very injurious, and that cancerous patients with ulcer of the stomach seldom vomit more than a small portion of their food; whilst Dr. Davis had pointed out that when cancerous disease of the stomach had existed for several months, the patient will often go from four days to ten without a passage from the bowels, and still the abdomen will be lank and empty.

The PRESIDENT thought that, if the ulcer had arisen from the abuse of ardent spirits, similar cases would be often met with; whilst the specimen exhibited was very rare and interesting.

*Horse-shoe Kidney.*—DR. WHITLA, house surgeon, Belfast General Hospital, read the following notes and exhibited a horse-shoe kidney:—

Maria C., aged seventeen, unmarried, certified to be labouring under gastric fever, was admitted into hospital on 3rd December, under care of Dr. H. S. Purdon. She had been in good health until five days previously; had menstruated only once, and that was five months ago. On her admission she was very weak and restless; face pallid; eyes staring, and respiration quick and panting; skin cool and dry; temperature  $98.1^{\circ}$ ; pulse rapid and weak; tongue moist and coated, with red papillæ projecting; but the most prominent symptom was persistent vomiting. In this state she remained for seven days, in which she was treated for an attack of gastritis; sinapisms over the stomach. Liquor bismuthi produced little change. Afterwards oxalate of cerium was given with benefit. On the 12th December some œdema about the ankles appeared. Her urine was examined and abundance of albumen found in it. There was no doubt now about the nature of the disease. The œdema spread rapidly. She got gradually worse. On the 15th January she lost power of one upper extremity. On the following day the corresponding lower limb was paralysed, and on the evening of the same day she died, being comatose for some few hours. The only food that remained on the stomach was buttermilk.

DR. FAGAN, from the appearance presented, considered that the cortical substance of the kidney was changed. He did not think a horse-shoe kidney a rarity, nor would its shape interfere with the due discharge of its functions.

DR. AICKEN thought the specimen was a curiosity. It had two pelves and connected ureters.



PROFESSOR DILL stated that he had often met with great irritability of the stomach in cases of Bright's disease. He had found the new preparation called "Koumis" succeed in allaying the irritability, and he supposed that the buttermilk that the patient took acted in a similar manner.

DR. H. S. PURDON said that the patient from whom the kidney exhibited was taken had been under his care in the hospital. The dropsy was not urgent; in fact, the vomiting was the most pressing symptom, and he had to treat symptoms. The oxalate of cerium succeeded in checking it. The only food she lived on for many days was buttermilk. He had tried the koumis in two cases of diabetes, but not in Bright's disease. He thought that the great irritability of the stomach and constant vomiting in this case were due to retained urea, which, as pointed out by Todd, often gave rise to retching and irritability of the mucous membrane of the stomach. He noticed that persons who had undergone the "Banting" system for the cure of corpulency were very liable to Bright's disease, from absorption of the fat that surrounds the kidneys, and he believed that the case under discussion was one of tubal or epithelial nephritis, affecting mainly the epithelium of the tubes and ultimately producing the large smooth white kidney. In addition, Dr. Purdon said that there existed slight hypertrophy of the left ventricle; and in these cases it had been found by Dr. Galabin that the arterioles corresponded in all respects to those of the granular kidney; and the state of the minute capillaries, often apparently fatty, with more or less hypertrophy of the muscular coats of the arteries, is due to increased arterial pressure, as proved by the sphygmograph. The epistaxis and hæmorrhage in the retina, or even cerebral hæmorrhage, are due to the degeneration in the walls of small vessels.

DR. WHITLA, in reply, said that the diarrhœa which had existed for a few days in the case might be explained on Johnston's theory, viz., that the retained urea was excreted by the intestines as carbonate of ammonia, and that the buttermilk would act as a neutralizing agent and sedative, which might account for the patient's craving for it.

*Post-Partum Hæmorrhage.*—DR. DAVID JOHNSTON brought under the notice of the Society, the case of a patient aged 21, whom he had attended in her first confinement. The labour was natural; but pains feeble; child still-born. He left her doing well at 2 o'clock p.m.; sent for at 6 o'clock p.m., and then found more or less oozing; emptied the uterus, and, when the loss had stopped, left. Again visited the patient at 10 o'clock p.m., being sent for; there was profuse hæmorrhage; pulse feeble, indeed nearly imperceptible; emptied the uterus of several clots, and, as the case was urgent, he injected

2 ounces of the perchloride of iron in 20 ounces of water, which, for the time, arrested the hæmorrhage. In about half an hour it recommenced; he then repeated the injection, and obtained the assistance of the President. The patient at this time was in great danger; tossing her arms about, sighing and restless. He injected the iron solution for the third time, 2 ounces to about 8 ounces of water. He thought that his first injection had produced little effect, as he had to do everything himself; but the others succeeded in stopping the hæmorrhage. Some surgeons look on the injection of iron as a dangerous remedy, but, in his case, the patient made a good recovery, there was no tenderness over the uterus at all. Ergot failed completely in this case.

DR. AICKEN said that if the placenta be soft, in the expulsion from the uterus, it sometimes tears and leaves part behind, and from this internal portion and its attachments oozing may take place. In a case of his, he had tried a sponge wrung out of vinegar and rolled round the uterus, which immediately brought on the uterine contractions.

DR. FAGAN noticed the beneficial effects of transfusion, in cases of severe post-partum hæmorrhage.

PROFESSOR DILL considered that the case under the notice of the Society was very interesting, and from what he could gather from the remarks of Dr. Johnston, the iron injection had suited best. He did not think that as yet we could pronounce a verdict, so to speak, on the value of the perchloride of iron. In this case, the hæmorrhage arose from want of contractility of the uterus, as was proved by the hæmorrhage coming slowly and gradually. In such a case the injection of iron is useful. If such a case presented itself in his practice he would, in the first instance, attack it by the application of cold. He seldom had seen a case of post-partum hæmorrhage that did not yield, if treated promptly, to the sudden application of cold, especially iced water, if it could be obtained, if not, then a handful of salt might be added to the water. In some cases the hæmorrhage could be anticipated by the character of the labour pains. If they are feeble and irregular, hæmorrhage may be expected. Ergot of rye, he thought, was useful only in some stages of labour, and ought to be given when the pains were feeble, giving rise to irregular contractions of the uterus. The drug was especially serviceable during the last three or four pains, just before expulsion. At this stage it certainly occasioned good contraction of the uterus, and he had little faith in ergot after the expulsion of the child. The circulation when quickened in those of a full habit of body—as shown by a quick pulse, often gave rise to hæmorrhage. In such cases previously to labour much could be done in the way of treatment, by quietness, by keeping the patient cool, and administering those remedies which quiet

the heart's action, as digitalis. Want of coagulability of the blood was another cause of hæmorrhage. In such cases gallic acid is useful. The Professor had formerly held the idea that when a patient died from *post-partum* hæmorrhage, it was generally the medical attendant's fault. Recently he had seen, chiefly in consultation, some three or four cases which were fatal, and he had changed his opinion, and chiefly for the reason that in all these patients there seemed to be a general varicose condition of the veins in the limbs and elsewhere, showing disease of the venous system. These cases were very dangerous, and he held that there was probably the same state of the uterine veins. In conclusion, he thought it might be worth while trying the plan of applying a tourniquet to the extremities, to stir up the blood in cases of *post-partum* hæmorrhage.

The PRESIDENT stated that during the 26 years he had practised his profession, he had been peculiarly exempt from cases of *post-partum* hæmorrhage, and had only one death from this cause. The case under notice was, what might be called, an extreme case, at the verge of death, and the treatment adopted was not only proper, but necessary. He had, in similar cases, compressed the aorta; but he believed that the injection of the solution of iron was more rapid and suitable. On examining the parts afterwards, the vagina would hardly admit the finger, showing the contracting power of the iron.

*Delirium Tremens.*—Dr. H. S. PURDON read the following notes of a case :—

Ordinary cases of *delirium tremens* are generally well within 48 hours, by inducing sleep. Occasionally, however, more obstinate cases arise. The following are the notes of one that has recently been under treatment in the general hospital.

Francis P., aged 43; married; by employment a publican; a powerful and well-made man, was admitted into hospital on January 7th, 1874, labouring under *delirium tremens*. He had been a "heavy drinker," and drank more than usual a short time before his being taken to hospital. The assistance of the police had to be obtained by his friends, so as to bring him to us. After his admission, he was quiet, but became very violent when anyone entered the cell in which he was confined. He was prevailed on to take a chloral draught at night, and slept 4½ hours. Next day he was very violent, dangerous, and delirious, had hallucinations of both sight and hearing, and was suspicious of everyone. The straight jacket had to be put on him; however, during the evening he became quieter, and the jacket was removed.

January 9th.—Again became very violent and delirious, so as to necessitate the use of the straight jacket. Tongue is now moist and

creamy; skin perspiring (hitherto it was dry); pulse 80, and soft; bowels moved; urine normal. Has not slept, although on the 8th and 9th he had chloral in  $\mathfrak{z}\text{i}$  doses, which had little or no effect, indeed it seemed to make him worse, and to increase the heart's action.

10th.—Much the same. Was ordered a mixture of opium and tartar emetic, and a purge of jalap powder.

11th.—Somewhat better; complains of "noise in the ears," and wishes his "brain to be examined," will submit, and requests his head to be "cut open," inclined to pray on every opportunity.

12th.—From this date till 17th, much quieter; bowels regular; appetite good; tongue clean; pulse fair. Occasionally quite rational, but always in a melancholy state. Ordered ergot, bromide of potassium, bromide of ammonium, iodide of potassium, in a bitter infusion.

17th.—When seen this morning he was very low. Muscles somewhat rigid; pulse weak; body bathed in sweat; won't open his eyes; complains of people talking to him. Ordered an enema of 10 grains of quinine and starch, and blister to neck. From the first (with the exception of one day) till 16th inst., he had  $\mathfrak{z}\text{iii}$  of whiskey given in milk, eggs and beef-tea.

18th.—Better; the stimulant (brandy) he had yesterday, during the sweating stage, roused him up, and stopped the sweating. Ordered to-day, a purgative of jalap and calomel, and the cold water douche. This profuse sweating may be due to vaso-motor nerve paralysis, and, as remarked by Anstie, prolonged alcoholic poisoning induces degenerative changes, due to direct chemical influence of alcohol upon the nervous tissue, causing paralysis of the nerves that preside over nutrition.

19th.—At the suggestion of Dr. Wales, under whose care he had been on former occasions, he was ordered some claret daily.

Jan. 24th.—Patient sleeps well, eats well, and seems in good bodily health, but still complains of hallucinations of sight and hearing, which, however, he knows perfectly well are false.

As in all cases of delirium tremens, there was in this patient well marked tremor and quivering of the tongue. The obstinate nature of the attack might be due to perverted nutrition of the brain, from the circulation of impure blood. As pointed out by Tanner, there is, in severe cases, often an increase of urea, with diminution of phosphates in the urine. Dr. Laycock, who has investigated the disease under notice, thinks that sleeplessness is not so serious a matter as is supposed in delirium tremens. No doubt there are two forms of this disease, viz. :—The acute and the chronic, the latter usually attended by mental and bodily depression. It is a curious and well known fact, that nearly all delirium tremens patients imagine that they see the devil, at some part of their illness. My patient thought that several devils surrounded him, urging him by voice to attack his attendants. Dr. Blandford writing on



auditory hallucinations (*Journal of Medical Science*, January, 1874) such as existed in the case I am describing, as well as hallucinations of sight, states that "the patients tormented by voices were more than commonly stout, and that hallucinations of sight belong to the acute rather than to the chronic stages of insanity." He defines the distinction between hallucinations and illusions to be "that an illusion is said to be a false interpretation of a real sound, while an hallucination is a false perception of a fancied voice or sound when there is no sound at all in reality." Dr. Barclay, on this subject, holds that "in the strict application of terms, the word hallucination implies that no object is present to stimulate that organ to which the idea formed in the mind is referred; while in illusions, existing objects which, in the first instance, produce the stimulus are clothed by the mind in characters more or less foreign to their true nature, and so blended with the sensation originally produced, as to give rise to the belief that the resulting idea is wholly derived from the external impression."

With regard to the treatment of delirium tremens, as debility is generally exhibited, nourishment, such as plenty of milk, eggs, and, in some cases, a moderate allowance of stimulants is, I think, necessary. I have remarked that when the patient is pale, thin, and not a confirmed tippler, and has not the greasy unctuous skin "reeking with volatile fatty acids," so characteristic of the drunkard, that Graves' plan of treatment by tartar emetic and opium, succeeds very well; however, opium sometimes stimulates, even when guarded, so to speak, with antimony. A good purgative is a capital preparation for this, or indeed any plan of treatment. Chloral is useful, and is now the popular remedy for delirium tremens, but, as remarked in this case, does not always agree, and often seems to stimulate the heart and brain. Red pepper in twenty grain doses has not succeeded in my hands, nor has bromide of potassium. I have had no experience of the tincture of digitalis in  $\frac{1}{2}$  ounce doses, as recommended by Jones of Jersey, nor of the hypodermic injection of caffeine, except in neuralgia. For the tremor and "unsteadiness" that remain after an attack of delirium tremens, the oxide of zinc in two grain doses thrice daily has done good, or, if the appetite is bad, quinine given with the ammoniated tincture of valerian, may be ordered. I believe that when renal congestion occurs the disease is very often fatal, ending in convulsions, coma, and death.

In conclusion, my chief object in bringing this case under the notice of the society, is not that I have anything new to offer in the way of pathology or of treatment, but to elicit the views of the members as regards the treatment, &c., of, I am sorry to say, a very common disease.

*Disease of Knee-joint.*—DR. MURNEY exhibited a diseased knee-joint which required amputation at the lower third of the thigh, and gave the following

history of the case:—The patient was a female, aged 24. About four years previously she had, as he believed from the description given, acute synovitis of the knee, which passed into the chronic stage, accompanied by occasional exacerbations. The swelling of the joint never completely diminished. Some four months since, ulceration of the cartilages commenced, and she then sought admission into the Belfast General Hospital. Last Saturday, January 24th, he amputated the thigh. She slept soundly the night after the operation; previously she had required large doses of anodynes; she is now progressing favourably. Upon examination the cartilages show extensive ulceration, in some places the cartilage of incrustation is completely gone. Moreover, the head of the tibia is eroded and greatly diseased. Some surgeons might think that excision should have been employed, but his experience, derived from the General Hospital, was opposed to that operation. The first case of excision of the knee-joint performed in the General Hospital was in 1850, by the late Dr. H. Stewart, and since then the results of the operation had been very unsatisfactory. Dr. Murney thought that excision of the elbow-joint gave much better results, and held that excision of the knee-joint was not a suitable operation, when the patient belonged to the working classes, whilst amongst those in a better position in society the results were more hopeful. In the former he preferred amputation; with regard to the production of a “stiff joint,” he thought that it might be tried in certain cases, and was better than excision.

DR. JAS. MOORE stated that he was not in favour of the operation of excision.

*February, 1874.*

*Notes of Cases of Fracture of the Inferior Maxillary Bone.* By RICHARD BARNETT, M.D., M.R.C.S.E.

IN 1836, a man named Macdonald, a coachman in the service of a gentleman residing a few miles from Belfast, returning one evening to his master, was waylaid and knocked down; when on the earth his assailant gave him a kick with a heavy shoe, which fractured the lower maxillary bone at the symphysis. The man was admitted into hospital, but, at the end of three months' treatment, union of the fractured surfaces had not taken place, and he was discharged. He was now placed under my father's care. There was a space of half an inch between the extremities of the fractured bone, one extremity being drawn in towards the base of the tongue. The four incisor teeth and alveoli containing them having exfoliated, the most anterior teeth that presented themselves were the right and left canine teeth.

A ligature of silk was tied firmly around the canine tooth of that portion of the bone which was drawn inwards toward the base of the

tongue by the action of its attached muscles. One assistant had charge of this—another, by means of a small instrument, like a fork, fitted to the neck of the other canine tooth, pressed that portion of the bone backwards, and so reduced the fracture as to leave the mouth free from anything that would interfere with the subsequent steps of the operation.

Bees' wax was softened and placed in a frame suited for taking impressions from the lower maxilla. This was carefully introduced into the mouth, and an impression taken of the space in the front where the incisor tooth *had been*, and of all the remaining teeth on both sides.

From the cast now taken a frame of silver was made, which fitted all the teeth of the lower maxilla, and dipping down from the apices of the canine teeth on each side to fit the space between these teeth. The silver capping-frame was now introduced into the mouth, and found to fit the crowns and exposed surfaces of all the teeth, and the space in front; but the man had no command over his jaws, and there was a strong involuntary lateral movement which was to be overcome, or the steps hitherto taken would have been useless. A strong silver bar was now soldered on the outside of the frame, and in a vertical position, so that when the mouth was closed, this bar passed between the buccal surface of the superior molar teeth and the cheek of that side of the frame to which it was soldered. This effectually prevented lateral movement in the opposite direction, which may be readily understood. A second bar was now attached to the opposite side of the frame, having the same relation to the superior molar teeth on that side. There could now be no lateral movement to either right or left side.

It now remained to make arrangements for feeding him, and that was managed in the following way. A silver bar was soldered to that part of the frame which covered the apices of the canine teeth, thus leaving a considerable space from the inferior margin of that bar to the frame covering the surface of the gum, and through this space he was fed. This bar also strengthened the frame, which at this part most required it, as it was more liable to injury at the central part.

This apparatus—consisting now of a frame, fitting all the lower teeth and gums, having two side bars in a vertical position, and one bar in the centre, passing from the apex of one canine tooth to the other, the space for feeding the patient being underneath this bar—was placed *in situ*, and the bandages passed round the vertex of the cranium in the usual manner. The patient, being directed not to interfere with the mechanism or disturb the bandages, went to the country.

At the expiration of six weeks he returned, and the bandages being removed and the instrument taken out of his mouth, it was found that he could open and shut his mouth at pleasure, the teeth on the right side and left being on the proper level. However, as yet the union was prin-

essentially cartilaginous, and would yield in a short time if left without support. The bones at seat of fracture were now rubbed, the one against the other, to cause irritation and excite more rapid ossification; and the frame being thoroughly cleansed—an operation it required very much—was replaced in the mouth, and the bandages applied as before. He returned in three weeks, and it was found that he had the perfect use of the lower maxilla. He was recommended to keep the frame in his mouth for another fortnight, and then to return it; and as he was going to seek a new service in a distant locality, he was requested to call on a medical gentleman who resided in that neighbourhood, and who was much interested in his case, but he neither returned the frame nor revisited any of those gentlemen who had exhibited so much interest in his case.

*Oblique Fracture of Lower Maxillary Bone.*—On Dec. 24, 1872, I was asked by Dr. James Moore to see a boy, Christopher White, aged fourteen, then in the General Hospital.

A fortnight before, the lad had fallen out of one of the port-holes of the “Gibraltar” training ship, a depth of 17 feet, into a boat alongside, and Dr. Moore being on board the ship at the time, had him removed same day to hospital.

The lower maxillary was fractured obliquely, internally from a point beneath the right lateral incisor, running outwards toward the left canine tooth, the left central and lateral incisor teeth and their alveoli being also carried away at the time of the injury, leaving an ugly fissure.

An external wound under the symphysis, communicating with the cavity of the mouth, was almost healed when I saw him. The smaller portion of the bone was drawn in and downwards by the depressor muscles of that side, and there was a considerable amount of motion between the affected parts.

In order to restrain the voluntary and involuntary motion of the parts, and, at the same time, give room for the introduction of food, I made a vulcanite splint. I passed a waxed silk ligature between the bicuspid teeth on the left side; and while an assistant, by means of this ligature, reduced the fracture, and, at same time, retained the base of the bone in position, I introduced a metal tray of suitable wire, filled with soft wax, into the mouth, and thus obtained an accurate impression of all the teeth and contiguous gums, including the seat of fissure, having also taken an impression in wax of the teeth of the superior maxilla. From these impressions, plaster of Paris models were made—those marked No. 1 now on the table.

A vulcanite splint was now made to fit accurately all the teeth of the lower maxillary—to cap them, and to descend over the gums and alveoli, both internally and externally. The superior surface of this splint was



carved out, so as to fit the grinding surfaces of the *superior* molars when the jaws were closed. On the left side a block of vulcanite was carried in a vertical direction, so as to fit the outside of the left upper molars when the mouth was closed, the object of this being to prevent all lateral motion.

On the 27th December this apparatus was introduced into his mouth. A piece of gutta-percha, previously softened in warm water, so as to take the mould of the external soft parts, was placed outside, under the base of the bone. This was semi-lunar in shape, and gave great support, some cotton wool being placed between the skin and the surface of the gutta-percha. Bandages were carried over the crown of the head, and around the neck, so as to steady all.

The following day I filled the upper surface of the splint at one part, so as to let the incisor and canine teeth on the right side through its surface. This made it more comfortable. The patient fed himself on soups and other fluid food, through the hole that can be seen by examining the models on the table. This space corresponds with the loss of teeth at the time of injury.

Two or three times a week the splint was removed from his mouth, cleansed with a tooth-brush, water, and soap, a mouth-wash, containing tincture of myrrh, being used at the same time.

Feb. 12, 1874.—The lad returned to his ship, able to do without the splint, and to give a very good account of a beefsteak; and the parts remain in a satisfactory condition at present date, fourteen months after fracture.

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

### CONTENTS.

THIRD SERIES, No. XXIX.—MAY 1, 1874.

#### PART I.—ORIGINAL COMMUNICATIONS.

PAGE

- ART. IX.—The History of a Bad Leg. By THE OWNER OF IT.  
 "A Lame Story," - - - - -

401

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. Recent Works on the Eustachian Tube:—1. The Function of the Eustachian Tube in its Relation to the Renewal and Density of the Air in the Tympanic Cavity, and to the Concavity of the Membrana Tympani. By THOMAS F. RUMBOLD. 2. On the Mechanism of Opening and Closing the Eustachian Tube. By C. S. F. YULE, B.A., 406
2. Family Thermometry: a Manual of Thermometry for Mothers, Nurses, Hospitalers, &c., who have charge of the Sick and the Young. By EDWARD SEGUIN, - - - - - 407
3. The Journal of Anatomy and Physiology. Conducted by Professors HUMPHREY and TURNER, - - - - - 407
4. Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., &c., - - - - - 408
5. The Principles and Practice of Surgery. By W. PIRRIE, F.R.S.E., 411
6. Lectures on Dermatology. By ERASMUS WILSON, F.R.S., - - - 414
7. A System of Oral Surgery; being a Consideration of the Diseases and Surgery of the Mouth, Jaws, and Associate Parts. By JAMES E. GARRETSON, M.D., D.D.S., - - - - - 415
8. Cholera: How to Avoid and Treat it. Popular Practical Notes. By HENRY BLANC, M.D., M.R.C.S., &c., - - - - - 417
9. The Simplicity of Life: an Introductory Chapter to Pathology. By RALPH RICHARDSON, M.A., M.D., - - - - - 420
10. The Anatomy of the Lymphatic System. By E. KLEIN, M.D., - - - 424
11. Schiller's "Don Carlos." Translated into English blank verse. By ANDREW WOOD, M.D., F.R.S.E., &c., &c., - - - - - 432
12. A Practical Treatise on Diseases of the Ear, including the Anatomy of the Organ. By D. B. ST. JOHN ROOSA, M.A., M.D., - - - 435

	PAGE
13. A Treatise on the Pneumatic Aspiration of Morbid Fluids: a Medico-Chirurgical Method of Diagnosis and Treatment of Cysts and Abscesses of the Liver, Strangulated Hernia, Retention of Urine, Pericarditis, Pleurisy, Hydrarthrosis, &c. By GEORGES DIEULAFOY, M.D., - - - - -	440
14. The Student's Guide to Surgical Anatomy. By EDWARD BELLAMY, F.R.C.S., - - - - -	442
15. Surgical Inquiries. By FURNEAUX JORDAN, F.R.C.S., - - - - -	443
16. Works on Electricity:—1. A Treatise on Medical Electricity. By JULIUS ALTHAUS, M.D. 2. Lectures on the Clinical Uses of Electricity. By J. RUSSELL REYNOLDS, M.D. 3. Clinical Researches in Electro-Surgery. By A. D. ROCKWELL, M.D., and G. M. BEARD, M.D. 4. Galvano-Therapeutics: A revised reprint of a Report made to the Illinois State Medical Society. By Dr. PRINCE, - - - - -	444
17. Manual of Chemical Analysis as applied to the Examination of Medicinal Chemicals. By F. HOFFMAN, Ph.D., - - - - -	446
18. Works on Diseases of the Skin:—1. Atlas of Portraits of Diseases of the Skin. New Sydenham Society. 2. On Lupus-disease of the Skin, and its Treatment by a New Method. By BALMANNO SQUIRE, M.B., - - - - -	447
19. A Handbook of Medical Electricity. By HERBERT TIBBITS, M.D.	447
20. The Effects of High Atmospheric Pressure, including the Caisson Disease. By ANDREW H. SMITH, M.D., - - - - -	448
21. On the Granular Cell found in Ovarian Fluid. By THOMAS M. DRYSDALE, M.D., - - - - -	450

### PART III.—MEDICAL MISCELLANY.

#### Transactions of the Medical Society of the College of Physicians:—

Case of Hæmorrhagic Sarcoma of the Pleura, combined with Extensive Pleuritic Effusion. By SAMUEL GORDON, M.B., F.K.Q.C.P.,	452
Coincident Mitral and Tricuspid Stenosis. By THOMAS HAYDEN, F.K.Q.C.P., - - - - -	459

#### Proceedings of the Dublin Obstetrical Society:—

A Case of Hydatids of the Uterus. By THOMAS DARBY, F.R.C.S.I.	464
On the Use of Perchloride of Iron in Post-partum Hæmorrhage (Adjourned Discussion), - - - - -	465

#### Proceedings of the Pathological Society of Dublin:—

DR. WILLIAM MOORE on Laryngeal Phthisis, - - - - -	475
DR. GRIMSHAW on Abscess of the Liver, - - - - -	476
DR. HAYDEN on Chronic Renal Disease, - - - - -	478
DR. HUGHES on Cirrhosis of the Lung, - - - - -	479
MR. TYRRELL on Spindle-celled Sarcoma of the Testicle, - - - - -	481
DR. STOKES on Enteric Fever, - - - - -	483
DR. A. W. FOOT on Pneumonic and Intestinal Phthisis, - - - - -	485
DR. THOMSON on Excision of the Knee, - - - - -	486
DR. HAYDEN on Bright's Disease, Œdema of the Glottis, - - - - -	487
DR. HAYDEN on Amyloid Degeneration of the Kidney, - - - - -	488

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

MAY 1, 1874.

---

### PART I.

### ORIGINAL COMMUNICATIONS.

---

ART. IX.—*The History of a Bad Leg.* By THE OWNER OF IT.  
“A Lame Story.”

LET me begin by thankfully acknowledging that for a lifetime—not far from “three score years and ten”—I have enjoyed remarkably good health, and have led an active, laborious life. With the exception of an attack of typhus fever thirty-five years ago, I have never been so ill as to be confined to bed for a week at a time, and even now I believe I have neither organic nor functional disease, except the “bad leg;” “only a little hipped,” as a waggish friend observed.

Until about twelve or fourteen years ago I was well able to walk or ride any distance with moderate fatigue, but at that time I felt a pain in my left leg in walking, not always, but occasionally, nor always in the same place, but sometimes very acute, so as to render walking very painful. Then it went off, and for some time I had neither pain nor difficulty in walking. For a very long time this pain was never in the hip-joint, or within some inches of it. Sometimes it would attack the knee-joint, sometimes the shin-bone or the thigh-bone, some inches below its neck. It always felt as if in the bone or joint, and not in the softer tissues.

As time went on the attacks of pain became more frequent, almost always coming on if I walked far, or stood for long, and the



pain did not quite go away by rest. Then gradually I found certain movements difficult and painful: I could no longer tie my shoes, or cut my corns, or divaricate my legs, which my friend, Dr. Adams, considers as characteristic of "chronic rheumatic arthritis." As he also told me that the disease is incurable, and that nothing does it much good, I may be considered a "purchaser with notice," as far as the future is concerned.

Instead of continuing this history in detail, it will be better to group under a few heads what I have to tell of the different symptoms:—

1. *The Pain*, which now may be regarded as an established institution. It varies much in intensity, from very acute stings on walking, or suddenly moving, to a dull ache, but I can hardly say that I am ever free from it, except when asleep. It is still rarely in the hip-joint, unless my leg gets a twist, but mostly down the femur. It is not increased by stamping the foot on the ground, or by a blow on the trochanter, but most acutely by separating the legs, or by kicking forward. It is increased by standing long, walking, or making a great effort, by sitting long on a hard or a low chair, the latter adding stiffness to pain. I do not know why the hardness or softness of a seat should make any difference, for the tuber ischii is not sore—but it does. A soft cushion is a great comfort. The easiest position in the day-time I find to be sitting on a high chair, with a soft cushion, with my feet on another chair about the same level. In this position it is easy to read, especially with the aid of a reading-table, and not difficult to write (as I am doing now), with the aid of a stiff writing pad.

It is not easy always to find an easy position in bed, but the most curious experience I have had is that if I go to bed (winter or summer) with warm feet I cannot go to sleep, for, in addition to the ordinary uneasiness, I have sudden twitchings of the leg, which render sleep impossible, until my feet are cooled by being quite uncovered. All my life, until within the last four years, I have slept upon a mattress over a feather bed. Now I find reversing the position of these two is a great improvement.

Upon the whole, I sleep very well, and if I find that the pain or starting is unusually bad, so as to keep me awake, I can always remedy that without subsequent inconvenience by twenty grains of the chloral hydrate.

I cannot say that I think the pain is much affected by the

weather and its variations, and, on the whole, I rather think that the pain is somewhat lessened during the last year.

2. *Locomotion*.—The first limitation of movement was in flexion of the joint. I found it very difficult to pull on my boots, or tie my shoes, so that I had to give up both, and be content with shoes with elastic sides, and even for those I require help now. This pain and difficulty on flexion speedily cured me of a love for low easy chairs, though it rendered a soft cushion on a higher chair quite as desirable.

Luckily I had given up riding before I discovered the impossibility of mounting astride a horse.

I am inclined to believe that the very decided "limp" in walking (and perhaps the "Grecian bend" also) is entirely owing to the pain. Dr. Adams says that shortening is not usual, and it may be that the appearance may be owing to the mode of standing, but practically the effect is that of shortening.

The walk is quite peculiar: it is not the "dot and go one" of a short leg; it is not the uncertain shuffle of spinal disease, nor the cautious, bent movement of a bad knee; nor is it the gentle attempt to save any irritation of the softer tissues. The leg is bent freely enough up to a certain point, and advanced readily enough, but when the foot is put upon the ground, and the weight of the body is thrown upon it, there is a sudden "wince" from pain, much as if you put your bare foot upon a pebble. It seems to me quite characteristic, and I think I could make a correct diagnosis of the cause of lameness from seeing a person walk. "A fellow-feeling makes us wondrous" sharp, as well as "kind."

This pain on planting the foot on the ground it is which renders a stick (or two, as Dr. Osborne maintained, and he was right) so necessary and helpful; at least, this feeling and another, which seems to me peculiar—I mean the feeling that you have no grip of the ground; that the least push would destroy your balance; something like what you feel on coming suddenly on smooth ice. I feel quite powerless to resist a high wind.

I cannot say that the disease has had any or very little effect on my health or spirits. I think my appetite has fortunately diminished with the impossibility of taking exercise, but every other function is as effective as one could expect at my age.

So much for the physical side of this disease—with the fine name, chronic rheumatic arthritis," which is more soothing than the

older one of "morbus coxæ senilis." But there is another aspect of the matter which I do not think has been noticed, and perhaps would not be likely to attract attention unless a doctor were the subject. I mean that in a chronic disability of this kind, a complete change—a revolution, in fact—of his usual habits must be effected; and a doctor labours under a great disadvantage in "seeing the end from the beginning," and being able, unfortunately, to anticipate what is before him. In his view—

"The leg, the leg, was the great event—  
Through every circle of life it went,  
Like the legs of a pair of compasses."

And in dwelling too constantly upon the evil days of coming incapacity, one may be—

"Jealous and envious and fretful by day,  
At night to his own sharp fever the prey;  
He lies, like a hedgehog rolled up the wrong way,  
Tormenting himself with his prickles."

Or, on the other hand, relying on better strength than his own, he may so modify his habits and tastes and pursuits as to enjoy life with a happy mind notwithstanding.

Perhaps the hardest thing to bear is the unchanging consciousness of inability, a permanent limitation of capability on all sides; and it must be confessed that this does press rather heavily upon one's spirits, but it does not last long, for within the charmed circle, outside which one cannot get, there is much that one can do and enjoy as well as ever.

I found that long walks had to be given up; then, that even moderate walks were too much; and, at last, that I must content myself with no walks at all. It must be the "contrariness" of human nature that on a fine spring day I have quite a yearning for a long walk.

Again, the suffering occasioned by sitting long on an ordinary chair at a table, or in a concert room, rendered it necessary to withdraw from all that class of social gatherings.

Travelling, also—that source of enjoyment—is nearly interdicted; for, although I manage very well in a railway carriage, yet an ordinary carriage, inside or out, is a kind of purgatory, and walking about seeing sights is impossible. My last attempt of that kind was at Windsor Castle and Buckingham Palace three or four years ago, and I shall not soon forget that. So strong is the feeling of locomotive incapacity that it appears like a dream that I should

ever have climbed a mountain; and even books of travels, especially such books as those of the Alpine Club, have acquired a sort of Gulliver or Munchausen character to me.

So, exactly as this limitation of movement increased, it had to be met by a change of habits, and some of my habits were fortunately capable of adequate expansion. After a time it became clear that night-work was out of the question, which, if a great relief to my leg, was quite the contrary to my pocket. But I am very thankful that I can go about during the day quite well enough, in a carriage with a high seat, and I can manage my house-business easily, and attend committees—so that I have work enough and interest enough after all. But as my powers of exertion are pretty well exhausted by 5 p.m., there remain many hours for which in-door occupation must be provided, and for this I found my love of reading the best resource. All my life I have read all sorts of books, at every opportunity, without being tired; and now that out-door amusements were impracticable, it became evident that to reading I must chiefly look for occupation and amusement. And, so far, it has not failed me; so many good books (and, indeed, so many foolish ones), so many newspapers, magazines, and reviews, are published, and there are such opportunities of getting books from libraries, that one can hardly be at a loss if one can only stretch one's sympathies a little.

And there are so many compensations that it would be a shame to allow mere personal disabling to cast a gloom over one's mind. For instance, although a doctor can foresee pretty much the course and consequences of such a disease, yet, as a matter of fact, it comes on so gradually, no day much worse than yesterday, that we need not encourage too much foreseeing, but let each day bear its own burden.

Then the kind of pain is peculiar—although I could hardly say that I am ever out of pain, and sometimes it is severe enough—yet it is a bearable pain. Unlike a pain in the head or stomach, it does not incapacitate you entirely; in a sense, you can “keep never minding it.” It does not prevent you thinking, or reading or writing; it in no sense invades your will or renders you less master of yourself. Its main effect is to teach you that your “strength is to sit still.”

And if it should become worse, and however prolonged it may be, one comfort for an old man is, that “the rest that remaineth” cannot be far off.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

#### RECENT WORKS ON THE EUSTACHIAN TUBE.

1. *The Function of the Eustachian Tube in its Relation to the Renewal and Density of the Air in the Tympanic Cavity, and to the Concavity of the Membrana Tympani.* By THOMAS F. RUMBOLD. Saint Louis: South Western Book and Publishing Co. 1873. Pp. 40.
2. *On the Mechanism of Opening and Closing the Eustachian Tube.* By C. S. F. YULE, B.A., Scholar S. John's Coll., Cantab., and Fell.-elect, Magdalen Coll., Oxon. *Journal of Anatomy and Physiology*, Nov. 1873. Macmillan, Cambridge and London.

DR. RUMBOLD endeavours in his paper to prove the correctness of six propositions, some of which are generally admitted. The first, however, is that during the act of deglutition the Eustachian tube is not an open passage into the tympanum. To prove this he assumes, as "self-evident," that if the Eustachian tube be open during deglutition, its patency from any other cause would not seriously interfere with the function of hearing. Having devoted the greater part of his essay to prove the disability of the organ produced by permanent patency, he considers his first proposition established. We think Dr. Rumbold cannot be acquainted with the view generally adopted in this country—viz., that during the act of deglutition there is a momentary impairment of hearing—a view which emphatically denies what he takes as "self-evident."

Mr. Yule has satisfied himself, both from experiment and rhinoscopic observation, that the Eustachian tube is open during deglutition. He concludes, however, both from careful dissection and observation during the act, that the tensor and levator palati muscles are not the active agents in effecting the opening, but that it is due to the combined action of the salpingo-pharyngeus and the palato-pharyngeus.

*Family Thermometry: a Manual of Thermometry for Mothers, Nurses, Hospitalers, &c., who have charge of the Sick and the Young.* By EDWARD SEGUIN. New York: Putnam & Sons. 1873.

THIS work of Dr. Seguin partakes much of the character of those pernicious productions known as works on domestic medicine, which generally result in demonstrating the truth of the old saying—"a little knowledge is a dangerous thing." While we have often acknowledged the value of the information derived by temperature observations taken in our absence by the nurses in attendance on our patients, just as we have of other information collected by intelligent nurses, yet we think but little good can be gained by such works as the present, which only tend to make mothers and nurses draw hasty, and often incorrect, and, it may be, dangerous conclusions from imperfect premises. We regret that Dr. Seguin's laudable zeal on behalf of clinical thermometry should have led him into the doubtful paths of "domestic medicine."

---

*The Journal of Anatomy and Physiology.* Conducted by Professors HUMPHREY and TURNER. Second series.—No. XIII. Nov. 1873. Cambridge and London: Macmillan and Co. Pp. 232.

THIS is a particularly interesting number of the Journal. It bears most unmistakable evidence to the increased energy already produced in British scientific research by the establishment of physiological laboratories, now so common in England and Scotland. In this number we have valuable papers both by teachers and pupils; and no less than five of the articles which are not continuations from a previous number, emanate from the physiological laboratory of the University of Cambridge. One of these is by Dr. M. Foster, while the others are by gentlemen in the position of students. Mr. Yule's paper on the functions of the Eustachian tube we notice elsewhere.

It is greatly to be regretted that in Ireland we have as yet no physiological laboratory. We trust, however, that the University of Dublin, now that they have had the good fortune to secure the services of so able an investigator as Professor Purser, will set the medical schools of Ireland a good example in this respect.

*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., Fellow of the Royal College of Physicians; Physician to the Hospital for Sick Children. Sixth edition, revised and enlarged. London: Longmans, Green, & Co. 1874. 8vo, pp. 858.

To those who are acquainted with the former editions of this work—and how few are not so?—but little need be said to commend this, the sixth edition, to their notice. In it, as the author explains in his Preface, are embodied the results of seven years more of clinical observation, the records of 743 more cases, and of 181 more *post-mortem* examinations. The groundwork of the book is thus based upon nearly 2,000 clinical records and nearly 600 *post-mortem* examinations. Notwithstanding, it forms by no means an unwieldy volume, and every page—nay, every sentence—is a witness to the care and thought which have been brought to bear upon the compilation of the work.

The classification of the subject-matter is extremely simple. Having given two introductory lectures on the study and treatment of children's diseases, Dr. West takes up in succession the consideration of the affections of the nervous, respiratory, circulatory, and digestive systems respectively; and he devotes the concluding portion of his book to the study of perhaps the most important class of the diseases of childhood—namely, fevers. In assigning a reason for the postponement of an inquiry into the last-named affections, the author unconsciously testifies to the soundness of his views respecting disease in the abstract. "These (*i.e.*, fevers)," he says, "I propose to consider last of all, because much of their danger arises from their complications, and to treat them judiciously you must be familiar with the diseases of the brain, the lungs, and the bowels." This is the outcome of the true *mens medica*, which looks upon essential disease as something under the control of fixed laws, arising, running a definite course, and subsiding after the lapse of an interval more or less prolonged—which recognizes the separate identity and separate danger of the local secondary affections setting in during the continuance of the parent malady; and which, lastly, in the light of this knowledge, pursues a line of treatment not hurtfully interfering with, but judiciously guiding, the course of the disease.

But Dr. West evidently possesses another qualification for success in the treatment of children and in the compiling of a

text-book of their diseases. We allude to the strong love for the little ones which everywhere crops up throughout these lectures. This is nowhere more clearly shown, however, than in the two introductory chapters, and the author himself lays stress upon the importance of the physician entering, for the time being, into the world of his youthful patients, with its joys and sorrows, and its *language*.

Speaking of this last-named, Dr. West (page 3) says:—

“Though the infant cannot talk, it has yet a language of its own, and this language it must be your first object to learn, if you mean ever to acquire the character of successful practitioners in the diseases of children. But, if you have not cultivated your faculties of observation, you cannot learn it, for it is a language of signs, and these signs are such as will escape the notice of the careless; *if you are not fond of little children*, you cannot learn it, for they soon make up their minds as to who loves them, and when ill they will express their real feelings, whether by words or signs, to no one else.”

What better advice, or more friendly warning, could be given to the young practitioner than that contained in the next sentence, which we shall take the liberty of quoting also:—

“There is, moreover, a certain tact necessary for successfully investigating the diseases of children. If, when summoned to a sick child, you enter the room abruptly, and going at once to your patient, you begin to look closely at it, while at the same time you question the mother or nurse about its ailments in your ordinary pitch of voice, the child, to whom you are a perfect stranger, will be frightened, and will begin to cry; its pulse and respiration will be hurried, its face will grow flushed, and you will thus have lost the opportunity of acquainting yourself with its real condition in many respects. Besides this, the child’s alarm once excited, will not subside so long as you are present; if you want to see its tongue, or auscultate its chest, its terrors will be renewed, and it will scream violently: you will leave the room little wiser than you entered it, and, very likely, fully convinced that it is impossible to make out children’s diseases.”

In the lecture on the “Treatment of Children’s Diseases,” we recognize the influence of the same guiding principle of love, and the hints given as to the *art of prescribing* for the very young are so illustrative of it, and at the same time so valuable, that we shall make two short extracts from the lecture:—



“The battle with a child to compel it to take medicine, to force it into a bath, or to give it an emetic, generally does far more harm than the remedy so administered can do good; and the many tears saved by it in the nursery are one of the strongest practical recommendations of homœopathy to the public.”

Again:—

“Many medicines, too, can be given without any trouble either to the child or to its attendants. A few drops of ipecacuanha wine will be unperceived in its drink, a little James’s powder may be concealed in some arrowroot or on a bit of bread and butter, or a dose of scammony may pass unnoticed in a little hot and sweetened milk; while, if tonics are needed, the saccharine carbonate of iron or the steel wine will seldom be refused by the most spoiled and most wilful inhabitant of the nursery. Your own ingenuity will suggest many other remedies which may be given without exciting suspicion, or at any rate without causing disgust; and, believe me, the doctor who brings smiles rather than tears into the nursery, he whom the children love most, the parents will trust most, and that love and trust will stand him much in stead when he has to combat serious illness.”

An index to *Formulæ* at the end of the work—comprised, let us add, in a page and a half—will show how admirably Dr. West has carried his advice in these paragraphs into effect, and will be a most trustworthy and useful guide to the inexperienced young physician.

Space would fail us did we attempt to enter at length into a consideration of the many topics dwelt on by the author. Nor, indeed, is this intended to be so much a critique upon, as a simple notice of, a work which has already, through the medium of six editions, won its way to fame, and vindicated its right to be regarded as above suspicion.

Its popularity is not confined to England; translations have long since appeared in German, Danish, Dutch, and Russian. In 1869 Dr. Blasi, of Rome, published an Italian rendering of the fifth edition, and Dr. Archambault, Physician to the Hôpital des Enfants Malades at Paris, is even now engaged upon a French translation of the present edition.

In the face of these facts it were, indeed, presumptuous to criticise. We have only to add that the book is published, in their usual first-rate style, by Messrs. Longmans, Green, & Co., to whose exertions the profession is indebted for editions of so many of the standard works on medicine and the allied sciences.

*The Principles and Practice of Surgery.* By WILLIAM PIRRIE, F.R.S.E.; Professor of Surgery in the University of Aberdeen; &c., &c.

PROFESSOR PIRRIE'S name has been so intimately associated with the subject of acupressure as a mode of arrest of surgical hæmorrhage, that we naturally turn to the chapter on this subject first in examining his third edition. We find that he has revised the modes of operating, and, as usually happens in dealing practically with any subject of the kind, that he has simplified the processes by reducing their number. He says:—

“I am the more anxious to do so (that is, describe clearly and briefly the best methods), as an imperfect knowledge of the best methods of acupressing vessels, and complete misconception regarding the proper manner of doing some of them, have led to failure and other untoward results. These untoward results, and the circumstances that obsolete modes are described in some excellent works on surgery, have all tended to retard the general adoption of this new means of arresting surgical hæmorrhage. At former periods in the history of acupressure it was deemed desirable to describe seven methods then in use; but the constant study of acupressure, both in hospital and in private practice, has thoroughly convinced me that three methods only are required, and that by one or other of these every vessel which it is possible to reach by ligature may be effectually secured by acupressure.”

We can only wish that in thus reducing the number of methods available, Professor Pirrie and Sir James Simpson, who were joint authors of the names to be used to distinguish these methods, had been more fortunate in their selection. Surely it would have been better to have used the simple numerals, as Sir James Simpson did in his publications on the subject, rather than adopt such absurd terms as “circumclulsion,” “torsoclusion,” and “retroclusion.”

The first of these methods is that well known as the third method of Simpson, in which a needle is passed beneath the vessel to be secured, and a wire loop is engaged on its point, and, after compressing the vessel, sufficiently secured by a single twist around the needle.

Torsoclusion is the familiar Aberdeen twist, the needle being introduced alongside the vessel, and twisted in the plane of the surface of the wound, so as to carry the open end of the tube round with it for a half-circle or more. In retroclusion, the third method,

the needle transfixes a small portion of tissue at right angles to the vessel, and, after passing over the tube, is made to describe a half-circle in a plane, vertical to the surface of the wound, and so carry the vessel round on its point—a method really the same as the second in its ultimate effect on the artery.

These, then, are the three methods finally approved of, a fact which in part bears out the statement of Simpson:—"That of the three methods of acupressure mentioned (by him), the one which is the most certain, and which probably will be most frequently followed by practitioners, is the third."

The next question of importance—"when is it safe to free an artery from acupressure?"—is still *sub judice*, although we should have thought experience of eight years in the hands of a surgeon who has "given it the preference in all suitable cases," would have been sufficient to decide it. Professor Pirrie, however, says, in reply to it, "time and experience have not yet done that work."

The only positive statement of the limits of time for the removal of the needle, we find, are those contained in the tabulated contrast of the ligature and acupressure, in which our author says the needle "is removable in an hour or two, or in one, two, or three days, at the will of the operator."

In this chapter Professor Pirrie says that:—

"Being Professor of Surgery in the University of Aberdeen, and one of the Surgeons of the Aberdeen Hospital, and it being one of the principal aims, and one of the greatest pleasures, of my life, to diffuse an accurate knowledge of surgery, and to excite enthusiasm for the study of that science, I considered it a duty to give acupressure a fair trial, and to endeavour to form, from personal experience, a just and unprejudiced judgment regarding it."

In spite of this little flourish of trumpets about himself and his effort to form a just and unprejudiced judgment, we find the Professor devotes all his attention to the comparison between acupressure and the ligature, while he disposes of torsion in the following passage, which shows pretty clearly that he has never tested its merits, and ignores what has been written by the best authorities on the subject recently:—

"Professor Miller's words on this subject are very emphatic, and, I believe, perfectly true—namely, 'The twisted portion of the vessel must slough and separate; the noose of a ligature is not more truly, nor to a

greater extent, a foreign body.' These considerations, together with the facts that Dupuytren reported unfavourably of torsion to the Institute, and that Velpeau, after giving torsion a prolonged trial, finally abandoned its use, have given me the impression that the revival of torsion in our day cannot justly be regarded as an improvement in this important department of practical surgery."

We must dissent from Professor Miller's opinion quoted above, and endorsed by our author, as simply contrary to fact; and we will not admit that the authority of either Dupuytren or of Velpeau is to be assumed as final. On the contrary, we hold that torsion deserves to rank on equal footing with the ligature and acupressure, and that the duty of a surgical teacher is to make his pupils expert in the use of these and every other means of dealing with hæmorrhage, that he may select his remedy as each particular case in practice requires.

Professor Pirrie does not, as it would appear, trouble himself much with the literature of surgery, or, at least, he ignores the writings and opinions of most competent authorities, even where they directly concern him. We find the following evidence on this point in his account of his seventh variety of dislocation of the shoulder:—

"VII. *Supra-glenoid.*—This is the only partial dislocation of the shoulder I have ever seen, and it is very rare. Of this accident, two examples only are recorded—the one by Mr. Soden, Junr., of Bath, and the other by myself. The possibility of its occurrence was first proved by a case which came under the observation of Mr. Soden, and afterwards by a preparation to which reference will be found in another page."

Professor Pirrie ignores in this passage the fact that Mr. Soden's case has been fully discussed, and its title to be considered an instance of dislocation set aside by competent authority. But further, he likewise ignores the criticism of most competent authority on his own case, while he advances neither knowledge of its history, or other details, in support of the idea that it is a true dislocation. His only reason for such statement is that the preparation "in all respects very closely agrees with the description and figure in the above case."

We now refer him to the following passage in the *Dublin Quarterly Journal of Medical Science*, May, 1853, in an article by the late Professor R. W. Smith:—



"I cannot, however, pass on without expressing my regret that, in several of the systematic treatises upon surgery that have recently appeared, in which Mr. Soden's case has been mentioned, no allusion whatever has been made to the elaborate analysis of it published in the *Cyclopædia of Anatomy and Physiology*." I allude more especially to the works of Professors Fergusson and Pirrie, both of whom have noticed the case in question as an example of what Mr. Soden termed it, without appearing to be at all aware that any doubt has ever been thrown upon it. Referring the reader, then, to the analysis above-mentioned for a complete refutation of Mr. Soden's view of his own case, I shall briefly detail the particulars of an analogous instance, of which I gave a full account at a meeting of the Surgical Society of Ireland in the year 1840." Professor Smith then goes on to describe the details of a case of chronic rheumatic arthritis of the shoulder, of which he furnishes two plates.

These remarks have been so long before the profession, and their truth has been so long admitted, that we are forced to conclude that Professor Pirrie is ignorant alike of them and of the disease they relate to. We cannot believe the other alternative—that he is knowingly misleading his class, for which, we presume, his book has been written.

---

*Lectures on Dermatology.* By ERASMUS WILSON, F.R.S. London: Churchill, 1873. 8vo. Pp. 295.

MR. WILSON deserves much credit for the collection of objects for a dermatological museum, and for his liberal endowment of a chair in connexion with it. As the first occupier of that chair it would be ungracious to criticise too closely the manner in which he fills it. Suffice it to say, that those who are already acquainted with his larger and well-known treatise on Diseases of the Skin, will find nothing new in this or any other of his later works, and little additional light cast on old matter.

Since the foundation of the chair, he has delivered four courses of lectures; the first was merely synoptical, the three latter consisted essentially of demonstrations of the specimens in the museum. The volume before us embraces the lectures delivered in 1871,

\* Professor Adams on Abnormal Conditions of the Shoulder-joint.

1872, and 1873; and as it covers about half the collection, we must make up our minds to the issue of at least one more bulky volume before the museum is exhausted.

---

*A System of Oral Surgery; being a Consideration of the Diseases and Surgery of the Mouth, Jaws, and Associate Parts.* By JAMES E. GARRETSON, M.D., D.D.S. Philadelphia: J. B. Lippincott & Co. London: Trübner & Co. Pp. 1,091. Illustrated with 354 wood-cuts and 17 steel plates. 1873.

THE voluminous work before us is rather a compilation, or ingenious stringing together of the various subjects comprised under the head of dental surgery, than the individual effort of the author. Dr. Garretson is, however, entitled to much credit for the generally practical nature of the extracts he has made use of; but the usefulness of the book, however, has been somewhat impaired by the poetical obscurity and wordy composition of the author, and would, in our opinion, bear considerable compression with advantage. At the same time we cannot but admire the research and energy that have brought together in this volume information hidden away in periodicals frequently inaccessible to the practitioner, and are glad to see the prominence with which the admirable results daily achieved by British skill in surgery are brought forward. Quotations from Heath, Liston, Fergusson, Syme, Colles, R. W. Smith, and Tomes, abound with frequency; while, on physiological points, the opinions of our German and French brethren are most exhaustively given. The author seems to have had considerable practical experience in the operations he describes, although British practitioners would consider many of them appertaining rather to the practice of the surgeon than to that of the dentist. We are willing, however, to give the author due credit for investing with interest a profession too frequently given over to the unprincipled and designing, and for bringing into harmonious working two kindred subjects. On the much vexed question of anæsthesia, American writers have hitherto differed considerably from us; but, strange to say, Dr. Garretson not only most fully and emphatically endorses the opinions and practice of Dr. Snow, but advocates the use of chloroform:—

“In oral surgery, where it is desirable to produce the profoundest and

most prolonged primary impression, chloroform, if it were without danger, is the anæsthetic most indicated and required. Indeed, in my own practice I find many cases where I do not seem to be able to get along without it, using it either alone, or in association with the ether.

“Therefore, because its use is so frequently necessary, the oral surgeon should make himself as familiar as possible with all that concerns its defects, as well as its virtues.

“In my own practice I never like to use chloroform but in conjunction with ether; not mixing them, as in the common chloric ether, but alternately, as the case seems to require or allow. Thus, employing the cone of sponge, I commence the process by pouring within it a quantity of ether, when it is gradually, and yet as rapidly as possible, brought to cover the mouth and nostrils. If now I find the pulse rapidly increasing, or even remaining fixed and steady, and particularly if undue cerebral excitability manifests itself, I drop into the cone ten or twenty drops of chloroform. This will generally be found to quiet the patient instantly. From this point I proceed, using principally the chloroform or ether, according to the result to be secured. If an operation is one of simple character and of quick performance, as the extraction of teeth, or the making of some simple puncture or incision, then I think we had better not risk anything with chloroform. If, on the contrary, it is some difficult and tedious case about the mouth, where, after commencing, the surgeon cannot well stop to re-administer the anæsthetic, then I take the risk of the excess of chloroform for the prolonged effect it yields, and the greater profundity of the impression it secures.

“The manner of the exhibition of the agent would have, as one would infer, much influence, as the result is concerned. Thus, of the various apparatuses that have from time to time been devised to assist in the use of chloroform, many are deserving of no better name than life-traps, and in this connexion one naturally finds himself wondering at the character of contrivances which, particularly in the earlier history of chloroform, were employed, even by the ablest men, in experiments directed to the testing of the general safety and results of the agent.

“I can never read of the white mice, and guinea-pigs, and cats, and bell-jars of these pioneers, without wondering that it should never have occurred to the experimenters that animals might as readily die from lack of proper respirable air as from the effects of chloroform.”

With respect to the use of nitrous oxide gas in surgery, Dr. Garretson does not seem to be in favour of it:—

“Except as its use in such speedily performed operations as the extraction of teeth is concerned, it would not seem that nitrous oxide

will ever be found applicable to oral operations (at least, as at present we know how to get the effects of the agent). In this opinion, I am aware, others may not feel disposed to agree with me, and it may very well be that I am wrong, as I have had little experience with the agent. Be this as it may, however, ether and chloroform appear so satisfactory and every way reliable, and at the same time are so convenient of use as compared with the gas, that I imagine they will continue to hold the supremacy."

The work is admirably printed on excellent paper, and the illustrations are generally most instructive, if we except a few "old familiar faces," whose release from duty in text-books would be a consummation devoutly to be wished.

---

*Cholera: How to Avoid and Treat it. Popular Practical Notes.*

By HENRY BLANC, M.D., M.R.C.S., Surgeon-Major H.M. Indian Army, &c., &c. Henry S. King & Co., London. 1873.

THE author commences with the rather enigmatical statement, that "cholera is not an imaginary something, without substance or being, heaven-born, floating about in the air, and wafted by uncertain winds." We may waive this flourish at the beginning of an otherwise interesting, and not by any means uninteresting, little book. We never heard of anyone who thought cholera "an *imaginary* something," and we think it scarcely necessary for the author to write even half a page negating such an "imaginary something" of an hypothesis. We also take leave to remind the author that the something which he seems in the above curious sentence to deny is "floating about in the air, and wafted by the uncertain winds," he assumes to be possible of propagation "through the air containing the particles of cholera." The distance wafted through may not be very great, but the statement on the second page surely rather detracts from the flourish on the first. The author professes himself a contagionist, and proceeds in a brief and graphic manner to sketch the progress of cholera from Allahabad and Benares in 1867 through its development and dissemination among the millions of filthy pilgrims who assembled at Hurdwar, on the banks of the Ganges, on April 1st, 1867, to bathe in, and even drink, the filthy waters of the bathing place shut off from the river for these purposes. On the 11th of April a great thunderstorm occurred, the rain which fell washing all surrounding filth



into the bathing place. On the 12th the bathing in and drinking of the dirty water commenced. On the 13th eight cases of cholera occurred. On the 15th all the pilgrims had dispersed, spreading the disease in every direction. The author then traces the spread of cholera in America, and adduces many well-known examples of contagion by drinking water, many of them quoted from Dr. Macnamara's work on Cholera.

Dr. Blanc states that "atmospherical and meteorological circumstances cannot generate cholera," but only affect its development and spread.

"Thus, in Calcutta, for twenty-six years :—

Three hot and dry months had	47,427	deaths.
Three cold and dry	23,632	„
Three hot and moist	11,354	„
The three transition	21,882	„

"With regard to rainfall for the same twenty-six years, we find that—

			Rainfall in Inches.	Cholera Deaths.
August -	-	-	14.4	3,440
February	-	-	0.42	9,346
March -	-	-	1.13	14,710
April -	-	-	2.4	19,382
May -	-	-	4.29	13,335
June -	-	-	10.1	6,325."

From the foregoing tables it is evident that "intense heat, heavy showers, lessened water supply, characterize the seasons during which cholera reaches its highest degree of maturity in its endemic area."

Our author proceeds then to study the question of prevention of cholera. That may be summed up in the words isolation, removal from infected localities, minute attention to cleanliness and disinfection, and, above all, a careful attention to the securing of a pure water supply, and, when secured, the keeping of the same pure. Dr. Blanc says in his concluding remarks on prevention :—

"Warned as we are by numerous and well-ascertained facts, ignorance and carelessness can no longer be deemed faults—they are crimes, which it is our duty to reveal and condemn, every one of us, whatever may be our position in life."

We wish that those to whom the sanitary care of our Irish towns is committed, would take the above quotation to heart, and act upon it before the next invasion of cholera.

Dr. Blanc, agreeing with Dr. Murray, objects to purgatives, even in the early stage of cholera, as likely to produce the sudden appearance of rice water evacuations and collapse. "During the former stage of diarrhœa few medicines were found more beneficial than opiates and anodynes."

Dr. Blanc insists on the stage of malaise, not that of diarrhœa, being considered as the real first stage of the disease, believing that precautionary measures, such as the avoidance of powerful purgatives, moderation in use of stimulants, and the avoidance of over-fatigue are the great means for moderating the future stage of the disease. His conclusions as to the treatment of the early stages of the disease are as follows:—"A few quickly repeated stools should, even in the absence of malaise, be at once checked. Opium in these stages is our best and most reliable remedy." Dr. Blanc recommends Dr. Murray's pills of black pepper, opium, and assafœtida as affording the best results. Our author again writes:—

"Opiates thus employed are but symptomatic remedies, but the experience of the many have decided in their favour, and the great importance of an early treatment in which opiates take the prominent part cannot be too well and widely known.

"Together with opium, diffusible stimulants, such as peppermint water, ether, brandy, infusion of chillies, &c., find their employ, but alcoholic stimulants, whether wine, brandy, or champagne, should be dealt with sparingly."

Dr. Blanc also recommends chloralum, and thinks favourably of antiseptic plans of treatment.

With regard to the stage of collapse, Dr. Murray remarks:—"Little suspecting that the means employed extinguish the last hope of the patient's recovery, it has been in protracted and helpless cases that the fatal practice of increasing the quantity of wine and the use of violent remedies have been resorted to in the anxious desire of rescuing a helpless case by desperate means."

We thus see that our author, who closely follows Dr. Murray, relies more on the preventive measures and treatment in the early stages than upon any means which can be adopted during the fully developed disease. The latter, it seems, can be combated only by relieving prominent symptoms, restoration of warmth, support of

strength, avoidance of exhaustion, and destruction or limitation of the cholera poison.

We recommend this little treatise as an excellent abstract of the present state of our knowledge with regard to cholera, especially as it appears in tropical climes.

---

*The Simplicity of Life : an Introductory Chapter to Pathology.* By RALPH RICHARDSON, M.A., M.D., Fellow of the College of Physicians of Edinburgh. London: Lewis. 1873. Pp. 118.

THE author begins by saying that—

“In preparing materials for an edition of Fletcher’s Pathology, the use of vague and uncertain terms in describing both healthy and morbid phenomena, has constantly thrown obstacles in the way of treating the subject. Before discussing the clear and lucid theory of Fletcher, who reduced all morbid actions to one, and the same law or general expression, it appears desirable that the terms which Fletcher employs when speaking of vital action should be explained; and as vital action implies life, this subject first demands attention.

“The following remarks on this subject, consist merely of some extracts from Fletcher’s Physiology, and a few old notes from his lectures.”

Now, we are at a loss, in reading this book, to know how much of it has been written by Dr. Richardson, and how much by Dr. Fletcher. It commences with fourteen pages, headed “Essay on the Simplicity of Life,” which is taken up chiefly with criticisms on the vague way in which words are used by Huxley, Gull, Beale, and Meyer. This, we suppose, is by Dr. Richardson. Then follow forty-eight pages, headed “From Rudiments of Physiology, by John Fletcher, 1836.” The text of this is accompanied by very copious foot-notes, giving the opinions of numerous writers, ancient and modern, on life, the vital principle, &c. The remainder of the volume is appendix, and consists of notes, some of which are taken from Fletcher’s Rudiments of Physiology, 1837; but whether these are the old notes from his lectures, or whether the notes in question are contained in the foot-notes, which throughout the volume occupy far more space than the text, we are unable to say.

There are at present, we are told, three principal theories of

life—1. The physical, supported by Huxley and Sir W. Gull, according to which life is only a form or mode of ordinary force. 2. The vital, supported by Dr. Beale, who considers life to be a power, force, or property of a special and peculiar kind, temporarily influencing matter and its ordinary forces, but entirely different from, and in no way correlated with, any other. 3. The natural, advanced by John Brown, and systematized and enforced by Fletcher, according to which life consists in the “sum of the actions of organized beings.”

Fletcher uses the term *property* to designate “a faculty, quality, or capability, such as irritability, sensibility, vitality, combustibility, elasticity, and so forth; all of which signify merely a susceptibility of excitement.”

“A *power* is variously understood as a stimulus, force, or agent, such as caloric, light, electricity, air, aliment, attracting force, &c., all of which signify a means whereby this susceptibility may be called into action.”

*Action* designates the phenomena resulting from the power and the property in co-operation, *e.g.*, irritation, sensation, life or vital action, combustion, reaction, gravitation.

According to Dr. Fletcher, organism or structure and life stand to each other in the relation indirectly of cause and effect. Organism gives rise to the development of a *property* called irritability or vitality, which, when acted upon by certain stimuli (*powers*), generally in more or less constant operation, produce those *actions* in the sum of which life consists.

This view is, as Dr. Fletcher says, not that which has been generally held. By most writers life and living action have not been looked on as identical, but life has been considered as a substantial principle, on which depend both organization and vital action.

The arguments in favour of the existence of a vital principle, or of life as a substance, are, according to Dr. Fletcher, chiefly three, *viz.*:—The impossibility of explaining without such an hypothesis either—1. The original organization of a living being; or, 2. Its characteristic actions when organized; and, 3. The incompatibility of the opposite opinion with a belief in a future state of existence. Each of these arguments is considered in the text, and in a foot-note either Dr. Fletcher or Dr. Richardson displays great research among the writings of the early philosophers on the nature of life. The opinions thus brought together do not appear to us



to be of much scientific interest, although they are just as valuable as most of the more recent speculations on the same subject. In answer to the first argument, it is admitted "that the chemical composition of organized tissues is quite distinct from inorganized compounds" (this would hardly be admitted now); "and, consequently, the first step towards organism must require powers different from those by which inorganized matters are formed. It requires Life, but not a Living Principle. It requires the Life, or living action (*Ζώνη*), of the thing *organizing*; not any Living Principle (*Ψύχην*) in the thing to be *organized*." In other words, every living thing is derived from some pre-existing living thing. "The organization, then, of the embryo, or the process by which a new being is formed, possessed of organs, however rude, and in virtue of its organism imbued with irritability or vitality, is the work of the parent." The question, of course, arises—whence the organization of the first parent? "With this problem the physiologist, in the strict sense of the word, has nothing whatever to do. The Almighty Creator—the first and the last—willed that not only the inorganic, but the organic kingdom of nature should exist; but how either the one or the other was originally called into being He alone knows who said—'Let there be light, and there was light.'" This is eminently satisfactory to the inquiring mind, who begins to ask why the book is called the *Simplicity* of Life?

In answer to the second argument, the author points to the analogy between the phenomena of life and those of chemical and mechanical action, "which we are contented to ascribe, not to any substantial principle of action, but to certain properties and powers resident in these matters (unorganized matters), the reciprocal action of which gives rise to what are called attraction and repulsion. And why need we hesitate to admit that similar, though not the same, properties and powers may, in organized beings, be competent, while they are in mutual co-operation, to effect those actions in which life consists, and which, of course, terminate on the cessation of this co-operation, as the ingredients of a chemical compound cease to be agitated when their affinities are satisfied?" It is further urged, that we know no more of the real nature of chemical and mechanical than we do of vital action; that we do not know any more why phosphorus, under certain circumstances, exhibits the phenomena of combustion, or ivory those of elasticity, than we know why certain organized substances exhibit the phenomena of irritability or vitality; and that, as we do not

attribute to the phosphorus or ivory any substantial principle of combustion or sensible motion, but only the properties of combustibility and elasticity, so we should only attribute to organized matter irritability or vitality, and not postulate any substantial principle on which its phenomena depend.

This is all very true, but again we ask, where is the simplicity of life? For it appears that, not only is life as deep a mystery as even Dr. Beale would have it, but that everything else, too, is a mystery.

With regard to the third argument, the author maintains that our disbelief in the *supposed* principle of life is no reason we should doubt the *real* principle of immortality; but it appears to us that the grounds for admitting one are about as strong as those on which we believe in the other. Science tells us nothing of either, and Revelation affirms both.

Having shown that there is no evidence in support of the existence of a vital principle, Dr. Fletcher, very logically, thinks it unnecessary to inquire whether such an entity is adequate to effect all that has been ascribed to it. The following quotation contains the gist of the theory, and shows what the author believes to be the connexion between Physiology and Pathology. "The notion of Life to be deduced from what has preceded is that it consists in the sum of the characteristic actions of organized beings, performed in virtue of a specific susceptibility, acted on by specific stimuli; and as this susceptibility and these stimuli, when natural, may be regarded respectively as the *Predisposing* and *Exciting Causes*, as it were, and the actions resulting from them as the *Proximate Cause* of HEALTH—so it is of some change in the first that every Predisposing cause, of some change in the second that every Exciting cause, and of some change in the last that every Proximate cause, of DISEASE severally consist."

Dr. Richardson disbelieves altogether in the applicability of the theory of the equivalence of force to explain the properties, powers, and actions of living beings; and maintains that it is only by a confusion in the use of words that the theory is made at all to apply to the actions of organized creatures. We must say that it appears to us that his criticisms on Meyer's work are the merest verbal quibbles, most of which show that he has altogether failed to grasp the true meaning of the theory of the Conservation of Energy. He himself does not at all explain whence the force so abundantly manifested in the living body is derived.

In conclusion, we have the astonishing assertions that heat and electricity are not objects, that they are not indestructible, that they are not endued with properties of any kind and that they have not the power of taking on different forms, but that they are, nevertheless, able to excite and cure disease; in proof of which we have a list, occupying eighteen pages, of diseases said to be cured or caused by heat, cold, electricity, passions of the mind, and irritation of some part of the body other than the seat of the disease. We think that in many of the cases given the evidence of causation or cure by the supposed agent would, on examination, be found defective, but, whether or no, the list seems chiefly valuable as a proof of Dr. Richardson's industry and patience.

From the poetical quotation with which the book terminates, we conclude that the author writes, not for this generation, but for posterity. To posterity we are happy to leave the responsibility of forming a judgment of his work.

---

*The Anatomy of the Lymphatic System.* By E. KLEIN, M.D., Assistant Professor at the Laboratory of the Brown Institution, London. I.—The Serous Membranes. London: Smith, Elder, & Co. 1873. 8vo. Pp. 98.

It is, we think, a good sign of the progress of scientific medicine in this country, that an histologist of such eminence as Dr. Klein should have found it to his advantage to come and settle amongst us. In all sincerity we welcome him, and also his book, the publication of which, in English, would seem to show that its author has made up his mind to remain in the country of his adoption. Dr. Klein is already well known to English readers by his papers in the *Microscopical Journal*, and by his share in the production of Dr. Burdon-Sanderson's *Handbook for the Physiological Laboratory*, but we believe the *Anatomy of the Lymphatic System* is the first separate work which he has published since his arrival in London.

The essay is divided into two parts; the first treats of the normal condition of the serous membranes—the second of their pathological conditions. The first part is divided into four chapters which deal with—1. The Endothelium of the Free Surface; 2. The

Cellular Elements of the Ground Substance; 3. The Lymphatics; and, 4. The Blood Vessels.

The most important point indicated in the first chapter is contained in the following paragraph (p. 3):—

“The endothelium of the free surface is, in the normal condition, not everywhere, a layer of flattened more or less hyaline cell-plates, but possesses in many places a different character, which may be described as follows: the individual cells are polyhedral, club-shaped, or even short columnar, their substance is distinctly granular, even in the fresh condition, their nucleus is, for the most part, like that of the ordinary endothelial plates, ovoid, sometimes spherical, clear, sharp-outlined, with a large shining nucleolus. In many cells, however, the nucleus is marked by a constriction, or is even perfectly divided into two.”

This kind of endothelium Klein calls *germinating* endothelium. It occurs in knobs or in folds or fringes. From the germinating cells there often arise, by budding, corpuscles, identical with those of lymph or pus, which sometimes exhibit distinct amæboid movements.

In some places, between the large common endothelial plates, branched or spindle-shaped granular cells are seen. The body of such a cell is sometimes placed at the point of junction of several endothelial cells, the processes running out between the latter, and becoming identified with the lines of intercellular cement substance, but sometimes these branched cells lie deeper than the endothelium, and come up to the surface only by one of their processes, which appears between the plates of endothelium. These form what are subsequently described as “pseudo-stomata.”

Chapter II.—When the omenta of fully-grown rabbits are observed, opaque tracts are seen of variable breadth, and which form a net-work. Some of these tracts consist of a row of patches of different sizes, which have more or less completely coalesced, and which are sometimes round but mostly oblong. In general the younger the animal the smaller and more isolated are the patches. In older animals they are larger, and have, to a greater extent, coalesced into tracts. These tracts generally follow the course of the larger blood vessels. “It may be deduced from this that in the omentum there is going on a continuous growth, and new-growth of opaque patches, which, by growing in length and coalescing in the direction of their longitudinal axis, form tracts” (p. 12).



In order to examine these patches and tracts it is necessary to stain the tissue with silver nitrate. This staining is effected with great difficulty, and failure in the preparation of the specimens is more common than success. In preparations where the basic substance remains unstained (positive preparations)<sup>a</sup> we see, when we examine small isolated patches which contain no blood vessels, a net-work formed of finely granular plates, which communicate with each other by shorter or longer processes of different breadth. In many of the plate-like enlargements a sharply defined nucleus is visible, which sometimes shows signs of division, and in whose neighbourhood the granular substance is more deeply stained.

In negative views, on the other hand, we see a system of clear lacunæ communicating by canaliculi, in which the granular cells already described are contained. In some places the cells lie together in groups, each group arranged about a common centre, towards which the cells are but little branched, while in the direction away from it they branch freely. These groups are produced by division of the branched cells. In some patches which contain blood vessels the branched cells are very numerous, and communicate freely with each other, and with the cells of neighbouring parts by their processes. Sometimes the matrix seems to have disappeared, and the intervals between the branching cells are occupied by lymphoid corpuscles. Where the matrix persists, lymphoid and other larger spherical cells are also seen; here they lie in the lacunæ with the branched cells, from which they are, in part, derived by proliferation.

The patches and tracts we have been considering are called "lymphangial patches (nodules) and lymphangial tracts." In the omentum of the rabbit two kinds of lymphangial structures are found:—

"1. Patches, the matrix of which consists of groups of ordinary, more or less flattened, more or less branched cells, which on the one hand multiply by division, in which way the patch increases in size, and from which, on the other hand, grow up lymphoid cells. The branched cells lie

<sup>a</sup> We may state for the information of those who are not acquainted with this histological method, that when tissues are treated with silver nitrate, two different appearances may be seen. In one case the ground substance is unstained, while the cells, and the canals in which they lie, are coloured (positive appearance); in others the ground substance, or inter-cellular matrix, is stained, and the spaces appear as clear marking on a dark ground (negative appearance). In this latter case the cells are usually invisible.

in the lymph-canalicular system, together with the lymphoid cells. At an early stage of development these patches do not contain a special system of blood vessels; at a later time they possess a special rich system of mostly capillary blood vessels. By growing in length these patches join so as to form whole tracts.

“2. Patches and tracts, the matrix of which consists of a reticulum, the meshes of which contain a variable number of lymphoid corpuscles; they are generally provided with more or less abundant blood vessels” (p. 17.)

The structure of the second kind of patches is pretty much that of the ordinary adenoid tissue of the lymphatic glands. For a reason that will appear subsequently, Klein calls the first kind peri-lymphangial, the second endo-lymphangial, nodules or tracts. The nodules and tracts are the analogues of the fat tissue of the omentum, into which, in some animals, they become transformed. The fat is accumulated, not in the lymphoid, but in the branched cells.

Chapter III.—The lymphatics sometimes accompany the large blood vessels, sometimes run independently of them, chiefly between and around the lymphatic patches. In both cases their wall consists of a single layer of endothelial cells; but those accompanying the blood vessels have valves, and are to be considered as main trunks, while those which run independently are without valves, and are to be looked on as capillaries. Sometimes a blood vessel runs for some distance within a lymphatic—is invaginated. Great care is necessary in order to see the lymphatics of the omentum or pleuræ mediastini, for, by very slight violence, they contract, and nothing is then seen but a row of spindle-shaped cells with a few lymphatic corpuscles.

The lymphatics are related, in two ways, to the patches and cords described in the last chapter. In the first kind of patches, or the peri-lymphangial, the lymphatic loses itself in the system of lacunæ and canaliculi, and the endothelial cells of its wall become continuous by branching processes with the flat cells which occupy the lacunæ. A different arrangement prevails with the second kind. We sometimes find a vein invaginated in a lymphatic, and this dilates so as to enclose all or several of the capillaries belonging to this vein in a common sac; or, sometimes, the lymphatic only accompanies the vein, and the system of capillaries belonging to the latter is inserted, “like the glomerulus of a Malpighian corpuscle,” into a blind sacciform dilatation of the lymphatic. In either case

the outside of the blood vessel, as well as the inside of the lymphatic, possesses a layer of endothelium. Sometimes nothing is seen between the walls but a few lymphoid cells, but in other cases there is an outgrowth from the endothelium, and the lumen of the lymphatic is occupied by a reticulum of branching cells, formed from these outgrowths, in the interstices of which lie numerous lymphoid corpuscles which have arisen by budding from the branched cells; in fact, the lumen of the lymphatic is occupied by a nodule (or, if it extend, a cord) of adenoid tissue, in the centre of which runs the blood vessel. Hence, such nodules and cords are called endolymphangial.

It is now generally recognized that the serous cavities are merely dilated portions of the lymphatic system, and that communications exist between them and the lymphatics which run in their walls. These communications are effected by openings, called stomata, between the endothelial cells of the serous surface.

Klein distinguishes two kinds of stomata—true and false. The true stomata are openings which lead by a vertical channel into a superficial or deep lymphatic vessel, or into a simple superficially-seated lymph lacuna.\* These openings are surrounded, and their tube lined, by cells presenting the characteristics of what was called, in the first chapter, *germinating* endothelium. The false stomata are not openings, but the processes of the deeper-seated branched cells, which were also noticed in the first chapter as cropping up between the plates of the surface endothelium.

The following is the account which Klein gives of the development of lymphatics in the serous membranes. In some of the buds, which spring from the lymph-canalicular cells, or in some of these cells themselves, a formation of vacuoles is observed. The vacuoles increase in size, until only a thin layer of protoplasm, often containing several nuclei, surrounds it. This thin wall then becomes differentiate into endothelial plates, and buds grow from the protoplasm of these into the vacuole, and finally become detached as young cells. The endothelial vesicles, thus formed, finally communicate with each other, either by fusion or by extension of the vacuoles through the protoplasmic processes which connect their walls. They communicate in a similar way with the neighbouring

\* A space formed by coalescence of several ordinary branched lacunæ, the flat cells of which now come to lie close to each other, and form an endothelium lining the deep surface only of the lacuna. The simple lymph lacuna lies immediately below the surface endothelium.

lymphatic capillaries, which may thus become longer, or have lateral sinuses or dilatations formed on their tube. If the vacuole extend along the cells whose branches come up to the surface between the endothelium, a false stoma may be converted into a true one.

Chapter IV.—In this chapter we find the process of development of blood capillaries described. In young patches, where few capillaries exist, it is seen that the walls of the latter are protoplasmic to a great extent, and that the endothelial markings can be followed only for a short distance near the origin and termination of the vessel. It is further seen that the branched cells of the matrix stand, by their processes, in direct continuity with the capillary walls. Again, several young capillaries end cæcally; the lumen becomes more or less suddenly narrowed, and the wall, which here is distinctly protoplasmic, having become solid, passes into an ordinary branched cell of the matrix. These branched cells also frequently show vacuolation, which, advancing towards the capillary, itself being hollowed out in another cell, finally opens into it. "Capillaries, therefore, are not only formed from previously existing capillaries by continuous excavation of the branched cells connected with their walls, but are also formed isolated in the branched cells themselves, becoming ultimately united with the former. Isolated vacuolation, for the purpose of the formation of blood capillaries, is a perfectly distinct process from vacuolation for the purpose of the development of lymphatic capillaries; for in the latter case *vesicles* are developed whose wall almost immediately shows an endothelial differentiation, even when these vesicles have not yet come into communication with lymphatic vessels; in the former case, on the contrary, this differentiation into endothelial plates does not take place for a considerable time" (p. 60).

In the second part the author treats in succession of the pathological changes of the superficial endothelium, of the cellular elements of the matrix, and of the lymphatic and blood vessels.

In severe acute general peritonitis, artificially induced, as by injection of ammonia into the abdomen, after from twelve to forty-eight hours, the serous membrane is found intensely hyperæmic, its cavity contains some bloody fluid, and there is abundant fibrinous exudation. The surface endothelium is loosened and removed over large spaces, and floats as detached membranes in the fluid. The cells, whether detached or *in situ*, are swollen, their protoplasm granular, and, in silver preparations, instead of the usual fine lines, dark coloured,



drop-shaped elements appear between neighbouring plates. The nuclei also exhibit signs of division.

If the inflammation be less intense, the shedding of the endothelial cells takes place to a much less degree, but they appear swollen, granular, and, in silver preparations, they become dark-coloured, and their nuclei appear in many places to be undergoing division; "in short, they possess more the appearance of epithelium." Where, in the normal state, germinating endothelium is found, there the most active proliferation is now seen to occur. This is most marked about the stomata and pseudo-stomata. From the proliferating endothelial cells amœboid corpuscles of various sizes detach themselves, and are found free in the exudation. All intermediate stages are found between the two degrees of inflammation.

If chronic peritonitis (artificial tuberculosis) be induced, a very active germination of the endothelium can be seen on the cords and knots, whose number and size have greatly increased. Also about the pseudo-stomata, buds and villous projections, formed of growing cells, are observed; these young cells are derived partly from the branched cell process forming the pseudo-stomata, and partly from the surrounding endothelium. About the true stomata, too, the germination is active and extends into the canal which communicates with the lymphatic capillaries, and "the young cells which result from the growth of the endothelium of the stoma fall partly into the corresponding lymphatic vessel, and partly into the abdominal cavity."

When the tuberculization is induced by subcutaneous injection, or by injection into the blood vessels, general tuberculosis is well advanced before any very marked proliferation of the peritoneal endothelium is seen; but when the infection is caused by injection into the abdomen, the changes in the peritoneum precede those in the other organs.

In acute inflammation, when the substance of the serous membrane is swollen by œdema, very important changes are to be seen in the cells of the matrix. The lacunæ are enlarged, the canaliculi less numerous, wider, and in many places provided with dilatations. The branched cells are granular, and show manifest signs of proliferation, not only in their nuclei, but also in their protoplasm. As a result of this proliferation, numbers of migrating cells are formed and accumulate in the lymph-canalicular spaces. The changes in the branched cells are thus the same as those observed by Norris and Stricker in the cells of the cornea.

In chronic inflammation, in many parts, the cells are more abundant, less branched, and their processes increased in breadth. In some places the cells lie so close together that they resemble an endothelium, and are separated from each other only by linear intercellular substance. The peri- and endo-lymphangial nodules and cords increase in size and number by an active proliferation of all their elements, branched cells as well as lymphoid corpuscles. In this way secondary nodules and cords are formed, which project over the general surface, and which are often covered with germinating endothelium. The pseudo-stomata, in many places, grow out as villous projections, bearing on their surface a covering of germinating endothelium. In the interior of such villi, fibrillar connective tissue bundles are subsequently developed, not by a change in the cells themselves, but, as Klein thinks, by an excretion or intercellular substance formed from them. The vacuolation of cells is very common in chronic inflammation, and in this way extension of lymphatics and blood vessels is effected, and false stomata changed into true.

The growth of a branched reticulum from the endothelial wall into the lumen of a lymphatic vessel, occurs to a much greater extent in inflamed than in healthy membranes, and so a great extension of endo-lymphangial nodules and cords is brought about.

Such is a very imperfect account of the contents of this book. We have had to omit all notice of many points of great interest, specially the varieties observed in healthy and diseased membranes in different animals, and in different parts of the same animal.

It would, of course, be mere idle presumption in us to offer any criticism on the value of the results arrived at by Dr. Klein, without having submitted his observations to the control of repetition, but the name of their author supplies a strong prejudice in their favour.

The manner in which this volume is brought out is deserving of great praise. The binding is handsome and strong, the paper and type good, and the plates (10 double-page, containing 54 figures) are, in point of artistic excellence, as near to perfection as anything of the kind we have ever seen. It is, therefore, much to be regretted that a more careful revision of the proofs was not made by some one thoroughly conversant with the English language. There are several errors in the text which are the result of mere carelessness. Thus, "Hartnack" always appears as "Hastnack;" we find "ovodi" for "ovoid," "pool" for "loop," &c. But in other cases there are

evidences of imperfect acquaintance with English. This is apparent throughout in the awkward construction of the sentences, which makes the book hard to read, even by one having some knowledge of the subject, and which will be sure to impair its popularity with those who are unfamiliar with German anatomical writing. We find, besides, numerous words and expressions which are not English—thus, “radiatory,” for “radiating;” “arterious,” for “arterial;” “intensive,” for “intense;” “take as,” for “take for;” “loses itself into,” for “loses itself in;” and many others which might easily have been avoided if the author had had the proofs carefully examined. The understanding of the text would also be very greatly facilitated if there were more frequent references to the plates.

We do not make these remarks in any captious spirit, but because, recognizing the many great excellences of the work, we are sorry to see in it even such minor blemishes as those we have noticed.

In conclusion, we have only to say that a welcome similar to that we gave to this volume will always be ready for any future works of Dr. Klein.

*Schiller's "Don Carlos."* Translated into English blank verse. By ANDREW WOOD, M.D., F.R.S.E., &c., &c. Edinburgh: William P. Nimmo. 1873. 8vo, pp. 331.

IT is not often that we have the pleasure of reviewing a book like this, which takes us out of the every-day round of medical science, and tempts us to make an excursion into the fair field of imaginative literature. We think, however, that we may with some pride claim Schiller as a brother *medicus*, seeing that he went through the regular *curriculum* of medical study, and even served for some time as an army surgeon; and we feel that we should be acting in a rather narrow-minded spirit, did we refuse a small space in our columns for a notice of this translation of Dr. Wood.

In his introduction Dr. Wood speaks with enthusiasm of Schiller, and indeed it must have needed a good deal of enthusiasm to make the laborious task which he has here accomplished a labour of love, as he appears to have thought it; but we cannot think that he has formed a just estimate of the place which Schiller occupies in literature. It is surely a somewhat vague way of speaking to name him in a breath with men of such very different *calibre* as Shakspeare, Goethe, Scott, and Byron. Whatever be Schiller's relative position as regards Scott or Byron, he certainly does not

deserve to rank with Shakspeare, or even Goethe. As compared with such men as these, he is quite small and common-place—a man gifted with but a finite portion of "the vision and the faculty divine." What Goethe said, and said truly, of his best work, that it was "altogether incommensurable," cannot be said of Schiller's. Good and valuable it is, no doubt, but it is work done on a distinctly lower level than that occupied by the author of "Faust."

"Don Carlos" is a good specimen of Schiller's powers, and, spite of the faults, which are apparent enough, lying, as they do, on the surface, it is a fine drama, and worthy of a painstaking and conscientious translator, as Dr. Wood, we think, is. The genius of Schiller was not so purely dramatic as that of Shakspeare. We never feel that his characters are so thoroughly human as those of our greatest English poet. Schiller is much more of an idealist, and we cannot help feeling that some abstract *idea* of character is often allowed to predominate over the naturalness of the character itself, just as the philosophic idea of the drama is apt to spoil the naturalness of the dramatic action. With Shakspeare we find the most improbable plots made probable by the naturalness of the action of the living men and women who take part in them. With Schiller even a probable plot is made to seem a little improbable from the comparatively feeble drawing of the characters. We are left in this very drama with a sense of annoyance at the catastrophe. In spite of the care with which it is worked out—perhaps *because* of this—we feel that the playwright, and not Providence, has brought it about. "*Quem Deus vult perdere prius dementat.*" But here the *Deus ex machina* is evidently Schiller himself, who pulls the strings of his puppets, and makes the *Marquis Posa* subside into idiocy just at the proper moment. No doubt it is a fine moral lesson, that when noble characters play with the edge-tools of deceit and dissimulation, they are likely to come to grief; but in a drama we demand a natural, not an artificial, *Nemesis*. As a confirmation of our remark that Schiller's power of character-drawing is comparatively feeble, we may cite his constant practice of supporting the speeches with a running commentary of the most minute and elaborate stage directions. Without this explanatory matter the text would sometimes be rather ambiguous. He even tells us in one place that a speech is to be spoken "*not ironically!*"

A drama of Schiller offers no small temptation to the English translator. The plot is usually interesting, the situation full of



dramatic effect, and the language often rises to noble bursts of eloquence. Schiller is, in fact, a master of rhetoric—this is one of his greatest powers—and his own earnest moral character, which he impresses upon all he writes, gives something of an epic tone even to his dramas. With all this, there is a limpid clearness in his language, which derives none of its dignity from mysterious obscurity. There are none of those *cruxes* which drive the translators of Goethe to despair. He seldom compresses a poem into a line or a phrase, as the men of high imagination often do. As compared with our own Shakspeare, his language seems bald, diffuse, without passionate concentration, almost without imagery. At first sight it seems an easy task to translate him; and indeed to give a good literal translation of one of his dramas into decent English blank verse, is one which demands care and industry, rather than high poetical genius. We think, therefore, that Dr. Wood has shown both wisdom and modesty in his choice; that his translation is, on the whole, very successful in point of literal accuracy; and that in places he has caught something of the spirit of the original. His verse is, however, in general, too stiffly constructed to reproduce the easy, graceful flow of Schiller's. It would probably require a second Coleridge to do this full justice; but we think that we have a right to demand from any translator something a little more musical than much of this verse of Dr. Wood, in which line follows line in monotonous succession, with scarcely a break in its rigid iambic form, and without much feeling for true *cæsura* or cadence.

As a favourable specimen of Dr. Wood's powers, we subjoin his translation of the speech of *Don Carlos* over the body of the murdered *Marquis Posa*:—

“Thou bleeding corse! forgive if I pollute,  
 Before such ears, thy noble character!  
 Yet this great scanner of mankind with shame  
 Is humbled, that a young man's intellect  
 Should have his grey-hair'd wisdom overmatch'd.  
 Yes, we were brothers! bound by nobler bonds  
 Than nature ever forged. His bright career  
 Was love—his glorious death was love for me.  
 When he with his esteem exalted you,  
 And when, with sportive eloquence, he play'd  
 With your proud giant spirit—mine he was.  
 You deem'd that you might master him—yet you  
 The pliant tool of his high plans became.  
 That I was made a prisoner was the work  
 Of friendship—the premeditated work.

He wrote the Prince of Orange, me to save—  
O God ! 'twas the first falsehood of his life !  
My life to save he rush'd to meet that death  
Which fell on him. Your favours you on him  
Most lavishly bestow'd—he died for me.  
You press'd on him your friendship and your heart ;  
Your sceptre was a plaything in his hands :  
He threw it from him—and he died for me !”

In conclusion, we think we may fairly congratulate Dr. Wood upon the zeal which he has evinced in devoting the leisure hours of a laborious profession to the production of a translation, which, if not satisfying all the requisitions of the highest criticism, has at least the merit of being an honest and decently accurate reproduction of the sense of the original.

---

*A Practical Treatise on Diseases of the Ear, including the Anatomy of the Organ.* By D. B. ST. JOHN ROOSA, M.A., M.D., Professor of Diseases of the Eye and the Ear in the University of the City of New York ; Surgeon to the Manhattan Eye and Ear Hospital, &c. New York: William Wood & Company, 27, Great Jones-street. 1873. 8vo, pp. 535.

DR. ROOSA's treatise confirms, in a remarkable manner, the opinion we expressed some time ago in this Journal respecting the leading position which the scientific medical practitioners of the United States are rapidly assuming, especially in ophthalmology and otology. It is worthy of note, that the two best works on otology of the present day—viz., Turnbull's and Roosa's—both emanate from the American press. Roosa's work is the production of an inquiring mind seeking after truth, and denouncing charlatanism and quackery ; it is a valuable and acceptable text-book, the great merits of which must commend it to every practitioner, and must insure its being read with profit and advantage by every one engaged in the special branch of which it treats. The historical portion in particular demands our warmest praise.

In ophthalmology Snellen's or Jæger's types afford a well-known and universally established standard for testing and recording the acuteness of vision ; in otology we have no such analogous standard for testing the hearing, inasmuch as there are very great difficulties in producing a sound-making machine which shall at all times produce uniform tones. Dr. Roosa suggests the adoption of a

formula described by Dr. Prout, of the Brooklyn Eye and Ear Hospital, as follows:—"For nearly three years I have recorded the hearing power as a fraction, the numerator of which is the distance at which the particular sound is heard, the denominator the distance at which it should be heard by an ear of good average hearing power. This denominator must vary according to the sonofactor used, and should generally be expressed in inches."

"For still further simplification, and that the method may be adapted to international use, I suggest the following abbreviations: A. D., *auris dextra*, instead of right ear, or R. E.; A. S., *auris sinistra*; P. A., P. aud., *potentia auditûs*, hearing power; V. vox, the spoken voice; V. S., *vox susurrata*, whispered voice—or simply S., *susurrus*, a whisper; H., *horologium*, the watch.

"If this system should become general, then the formula P A, A D, H,  $=\frac{12}{36}$ , would to all otologists represent the fact that a watch that should be heard at 36 inches was heard by the right ear of the patient at a distance of 12 inches; the formula P A, A S, V S,  $=\frac{6}{36}$ , would mean that the whispered voice was heard by the left ear at 6 inches that should have been heard at 36 inches."

It has long been an established fact that there are certain aural diseases peculiar to the insane; amongst these othæmatoma, hæmatoma auris, or vascular tumour of the auricle has given rise to considerable discussion as to its nature and origin, some considering it due to violence, others as of spontaneous origin. Dr. Thurnam, of the Wilts Asylum, stated many years ago that it was much more frequent in former years, when restraint was extensively used. Dr. Laycock concludes that the state of circulation and nutrition in the structures of the auricle coincides very often with similar conditions in the membranes of the brain and in the brain substance; and this correlation would no doubt explain the occurrence of the sanguineous tumours. Dr. Brown-Séguard produced hæmorrhage beneath the skin of the auricle by sections of the rectiform bodies in animals, and he believes that disease of the base of the brain—which is, however, not always attended by insanity—is the cause of hæmatoma auris. Dr. Hun, of Albany, N. Y., who has had very great experience of these cases, concludes that idiopathic othæmatoma is a symptom of insanity, and he would consider any person having such a tumour upon the auricle, even if sane, as a person to be carefully observed as to cerebral symptoms. Dr. Roosa's chapter on the subject is very interesting. He says that—

“*Gudden*, a German writer and physician for the insane, quoted by Virchow, has shown that the auricles of ancient statues are very frequently ornamented by tumours resembling the vascular effusions seen among the insane. In the gallery at Munich the head of Hercules has such ears. These misshapen auricles are the typical marks of the ancient boxers or pugilists. Such fighters wrapped their hands in leather, and thus armed, struck the ears of their antagonists; consequently, in the figures of Hercules, Pollux, and other classical fighters, a deformed auricle is a regular appearance. Other historical personages—the Trojan Hector for example—are represented as having othæmatomata.”

And he concludes:—

“First. That there are two distinct varieties of othæmatomata: Traumatic and Idiopathic.

“Second. That the idiopathic is much more common among the insane than among others, but that identically or nearly the same affection does occur among the sane. It is probable, however, from Brown-Séquard’s experiments, that the affection is caused by some lesion of the base of the brain, so that although persons suffering from vascular tumour of the ear may not always be insane, they generally have brain disease.

“Third. The traumatic form differs from the idiopathic in being a simple extravasation of blood from vessels ruptured by violence. In such cases the deformity resulting from the spontaneous effusions does not occur unless among professional pugilists, where the violence is frequently repeated, and the auricle, from repeated hæmorrhages, assumes a shape like that resulting from a true othæmatoma.”

One of the most distressing complaints for which the aurist, as well as the physician, is consulted, is tinnitus aurium, very often an accompanying symptom of some aural disease and deafness, but sometimes apparently not dependent upon aural affection. So intolerable do these noises sometimes become that the sufferer puts an end to them and his existence at the same time; a melancholy case of this kind occurred a few years ago in the neighbourhood of Dublin. Dr. Roosa says—

“It is the opinion of Schwartze, of Halle, a very careful and competent observer, that subjective aural sensations, which are caused by demonstrable affections of the ear, may, in predisposed persons, especially when there is any hereditary tendency to mental disease, become the direct cause of aural hallucinations, that may accelerate the outbreak of a disease of the brain. He mentions a case where in his opinion, and in that of one of the physicians of the Insane Asylum at Halle, a threatened attack



of brain disease was prevented by treatment of the ear. In some cases insane persons who suffer from aural disease distinguish its tinnitus from these illusions or hallucinations.

“Dr. Koppe confirms this view, and shows that in some cases hallucinations disappear after treatment of the ear.

“I have elsewhere reported<sup>a</sup> a case of the suicide of a professor in one of our educational institutions, who consulted me on account of impairment of hearing, but more especially on account of tinnitus aurium. He said, on leaving the consulting-room, that, if he felt sure that I was correct in my opinion (that he would not get great relief from this very trying symptom, tinnitus), he would put an end to his existence; which he did a few months after, by blowing out his brains. During this last summer, a gentleman, a public-school teacher, consulted my associate, Dr. Charles S. Bull, in regard to a suppuration of the ear, which caused considerable impairment of hearing and great tinnitus. He was exceedingly depressed and annoyed by the tinnitus. It is said that he committed suicide on account of the depression caused by this state of his ears. There can be no doubt but that this symptom is one of the most distressing that can befall a patient, and that in some cases it is the provoking cause of suicide. Again and again I have satisfied myself that the great depression, which is the rule in persons whose hearing is impaired, was due entirely to the aural disease.

“Dr. O. D. Pomeroy,<sup>b</sup> of this city, examined sixty lunatics at Blackwell's Island Lunatic Asylum, and he found disease of the ear in many of those who suffered from what may be called aural hallucinations, although this proportion was not as large as stated by Schwartze and Koppe.

“Dr. C. E. Wright<sup>c</sup> publishes a case of a patient in the Indiana State Asylum for the Insane, who attempted to destroy herself by putting a steel button in her ear. The patient was discharged from the hospital as having recovered her reason, but became nervous and despondent, until she was relieved by the removal of the button; and a dread of insanity and of sudden death, from which she suffered, then also disappeared.

“Von Tröltsch speaks of confusion of the intellect, an inability to keep up a connected line of thought, as a subjective symptom of chronic aural disease, and I am enabled to verify this opinion. Over and over again have patients with chronic diseases of the middle ear, not suffering from pain but from tinnitus, voluntarily informed me that these noises, together with the impairment of the hearing, had a great effect upon their mental powers. On the other hand, I have seen cases where most successful men, such, for instance, as distinguished general officers in the

<sup>a</sup> New York Medical Journal, August, 1869.

<sup>b</sup> Transactions of the American Otological Society, Fourth Year, p. 46.

<sup>c</sup> Indiana Journal of Medicine, November, 1871.

army, and distinguished writers, have suffered from boyhood with chronic inflammation of the middle ear and tinnitus aurium."

As our knowledge of pathology becomes more accurate and extended, so does our nomenclature become more rational and simple, and the work now under consideration bears this amply out. Diseases of the middle ear, which form so predominant a feature in the causes of deafness, are classified by Roosa into suppurative and non-suppurative, and the latter subdivided into catarrhal and *proliferous*, the latter term being borrowed from the German *Wucherung*, or proliferation.

Our thanks are due to Dr. Roosa for his simplification of nomenclature, as well as for his historical notes throughout his work.

The author again directs attention in this work to the dangerous sequences of Weber's nasal douche, to which we referred in a former number of this Journal. He says that the use of this douche gives rise to acute aural catarrh and necrosis of the middle ear, as well as general constitutional disturbances, depositions of pus in distant parts, &c. It is right to mention, however, that Dr. Elsberg and others combat and negative these views.

We regret that the small space at our disposal forbids our entering fully into the subject of paracentesis of the membrana tympani and division of the tensor tympani; our own limited experience is not favourable to the former operation, nor have we as yet seen any case which did actually derive benefit from this procedure where other treatment had failed to afford relief. We give Dr. Roosa's conclusions—

"My own experience in perforations of the membrane tympani has been chiefly in the manner of Schwartz and Hinton; that is to say, I have made simple paracenteses or openings into the membrane, and followed them up by treatment of the diseased membrane of the middle ear. Paracentesis seems to me a perfectly safe operation; it is comparatively painless, and is certainly an adjuvant in the treatment of chronic non-suppurative inflammation. My results are not as good as Professor Schwartz's, perhaps because I have been in the habit of treating many of the cases that he treats by paracentesis, by simpler means.

"It should be added to what has been said that the paracentesis that is performed in chronic cases should be a larger one than the puncture made in a bulging membrane, to give exit to blood or pus.

"From the experience which I have had, and from a careful consideration of the recorded experience of others, I think we may conclude—

"I. That paracentesis, or incision of the drum-membrane in chronic

non-suppurative inflammation, is by no means a dangerous or painful procedure.

“II. That its chief value is in furnishing a means of treating the lining of the middle ear.

“III. That it may properly be performed in cases of chronic proliferous inflammation that are still advancing in spite of local treatment through the Eustachian tube.

“IV. Division of the tendon of the tensor tympani, and division of the adhesions existing between the membrana tympani and the walls of the cavity of the tympanum, are operations that deserve a trial, in cases of chronic inflammation of the middle ear, with symptoms of increased auricular pressure, not relieved by a fair use of the ordinary means.”

*A Treatise on the Pneumatic Aspiration of Morbid Fluids: A Medico-Chirurgical Method of Diagnosis and Treatment of Cysts and Abscesses of the Liver, Strangulated Hernia, Retention of Urine, Pericarditis, Pleurisy, Hydrarthrosis, &c.* By GEORGES DIEULAFOY, M.D., Gold Medallist of the Hospital of Paris. London: Smith, Elder, & Co. 1873. 8vo, 394 pages.

IN Part I., which is a kind of introduction, the author attempts by a short historical sketch, to establish the priority of his discovery of aspiration, properly so called, as all the instruments used prior to his lacked the two great essentials of aspiration—namely, (*a.*) a previous vacuum, and, (*b.*) extremely fine needles. By a “previous vacuum” he means one made before the needle is introduced and applied to the part the moment the orifice in the point of the needle has penetrated the tissue; thus it is a capillary needle, carrying a vacuum at its point, which becomes an intelligent force, indicating the moment when fluid is reached, and thus it forms an invaluable aid to diagnosis, “it being always possible,” he states, “to search for a collection of fluids without any danger, whatever may be its seat or nature.”

As a mode of treatment he claims for it the monopoly of removing all liquid collections from the body; as being the most perfect means of injecting, and thus preventing re-accumulation, he considers its remedial value complete.

In Part II. he details, at considerable length, the various forms of diseases, suitable to aspiration, occurring in organs, giving numerous cases illustrative of his conviction of the complete harmlessness and efficacy of the instrument.

The liver he punctures in all directions in search of cysts or abscess, without the least inconvenience to the patient. In retention of urine, the harmlessness of the aspiratory acupuncture of the bladder makes him lay down the rule, "that in a case of retention, whatever be the cause, if systematic catheterization have twice failed, we must have recourse to aspiration." We observe, however, that some of the twenty cases he details, as relieved without accident, appear to have died with symptoms rather like those of acute peritonitis, which he attributes to some other cause.

In ovariectomy and hydrocephalus, the author's experience does not lead him to be quite so sanguine about the result of aspiration.

On the basis that the accumulation of gases and fluids in a strangulated piece of gut is "most frequently the sole cause that prevents reduction," and that the harmless operation does not interfere with any ulterior curative measures, he strongly recommends the use of aspiration, "in all cases of strangulated hernia," not waiting till taxis has failed, but using the instrument as a most direct and efficacious auxiliary to the reduction of all hernia.

Part III. treats of the removal of fluid collections from serous cavities, among the first of which the author discusses pericardial effusion, where he thinks the aspirator fills the chasm which has so long been felt in medicine. With reference to pleural effusions, he states he has found a simple and harmless mode of certain diagnosis of the existence of the seat and nature of the fluid. In the practice of such investigations he looks upon puncture of the lung as an insignificant circumstance; and as it is usually congested when a question of diagnosis arises, the aspiration of a few grammes of blood from the organ is rather salutary than otherwise. The operation ought to be performed easily, not waiting for fever or dyspnœa, but as a means of anticipating the accumulation, which should never be allowed to attain 500 grammes in adults, and much less in children. If frequently repeated operations do not dry up the fluid, injections must be had recourse to, and the pleura washed out two or three times a day.

Hydrarthrosis of the knee he considers singularly well suited to this mode of treatment. No matter what may be the cause of the effusion, or whether it be serous, sanguineous, or purulent, the treatment is—repeated aspiration.

Part IV. treats of the evacuation of all forms of fluid collections in cellular tissue; and each form of abscess, the author seems to think, possesses some peculiarity indicating the use of the aspirator.



Part V. consists of an explanation and description of the various kinds of instruments now in use.

The aspirator must take a high place among the modern improvements in medicine, and, doubtless, will soon be in the hands of every practitioner, but we fear that its inventor claims, in the work before us, too exclusive a right to the treatment of fluid collections in the body, and also exaggerates the harmlessness of the instrument, for there is no doubt other methods can be successful, and even aspiration may be followed by serious accidents. The author gives but few directions to be attended to in the performance of this operation, which he considers does not require any anatomical knowledge. Four precautions he impresses on the reader in nearly every chapter:—(1.) To assure oneself of the permeability of the needle by a stream of water and the silver wire; (2.) To set the instrument before using it—*i.e.*, make the vacuum previous to the insertion of the needle; (3.) Apply the power of the vacuum the moment the needle has entered the tissues. (4.) Keep the patient perfectly quiet after the operation, avoiding all kinds of physical examinations.

The English is clear, and, on the whole, creditable to a foreigner, for it appears that Mons. Dieulafoy did not trust the extolment of his invention to a translator, but prefers to write the work himself, which fact accounts for the peculiar sense in which some words are used, as *accidents* for symptoms, *development* for enlargement, *inspiration* for respiration, and also for the sensational tone assumed in describing the pitiable attempts made to evacuate certain fluids before the invention of this new method.

---

*The Student's Guide to Surgical Anatomy.* By EDWARD BELLAMY, F.R.C.S. Pp. 297. London: J. & A. Churchill, New Burlington-street.

WE notice the appearance of this book with pleasure, as an important addition to the literature of our profession. There are some who hold very strong opinions as to what is described as the over-teaching of anatomy in the schools. The student is required to make up a mass of details of the minutest character, which to the average practitioner are useless in after-life, and are, indeed, forgotten as soon as the pupil has passed from the grinder's care. Surgery, medicine, and physiology hold an inferior place in respect of the time devoted to gaining a knowledge of them. Indeed, it is

not uncommon to find men filled with the most curious anatomical lore, who are ignorant of the rudimentary principles of the subjects with which they will have most to do in practice. Such, in a few words, is the contention of those to whom we refer—with what justice we need not at present discuss. But even they will admit the absolute necessity of students being acquainted with those branches of anatomy which are connected with the ordinary run of surgical and medical cases. No one may hope to be a scientific surgeon who lacks knowledge of the muscular, nervous, arterial, and articular systems of the body; and it is precisely in these points that Mr. Bellamy hopes to afford instruction to the student of the present day. The field is a comparatively unoccupied one, for the translation of Galton's work is the only publication on the subject of any pretensions here.

So far as it goes, Mr. Bellamy's book is excellent, and it is sure to attain a speedy popularity. Regional anatomy is one of its prominent features. The surface markings, which indicate important parts, and serve as guides to the surgeon, are carefully given, and will be found of great service. Of course, in a work of this kind we must not expect elaborate descriptions of operations, but the author has given suggestive outlines of the more important. The parts divided in various forms of amputation are accurately enumerated, and the several operations for the ligature of arteries are appended to a description of the relations of these vessels. We are surprised, however, to observe the cursory manner in which the author has treated the subject of fractures. In such a book we should look for a fuller explanation of the effects of muscular action in producing displacements in these affections. There are some other defects of a much less important nature, but we shall hope to see them corrected in any subsequent edition. Even as it stands, however, the student will find the book a most valuable addition to his collection, enabling him to see much of the practical application of the knowledge acquired by months of laborious study in the dissecting room.

---

*Surgical Inquiries.* By FURNEAUX JORDAN, F.R.C.S., Surgeon to the Queen's Hospital, Professor of Surgery at the Queen's College.

THIS book consists of some thirty illustrations, collected in eight plates, and a letterpress of twenty-eight pages. When this much

is stated, we have said nearly all that the book requires, for we have rarely seen a more evident example of simple book-making.

The text consists of a rambling series of notes on all sorts of subjects in surgery, arranged in no particular order, and seemingly constructed chiefly with the view of advertising the originality and skill of the author. The illustrations are calculated, like the bizarre trade-marks of an advertisement, rather to attract the attention of the public to the book than to convey information to the profession.

---

### WORKS ON ELECTRICITY.

1. *A Treatise on Medical Electricity.* By JULIUS ALTHAUS, M.D. Third edition. 1873. Pp. 729.
2. *Lectures on the Clinical Uses of Electricity.* By J. RUSSELL REYNOLDS, M.D. Second edition. 1873. Pp. 116.
3. *Clinical Researches in Electro-Surgery.* By A. D. ROCKWELL, M.D., and G. M. BEARD, M.D. New York. 1873. Pp. 72.
4. *Galvano-Therapeutics.* A revised reprint of a Report made to the Illinois State Medical Society. By Dr. PRINCE. Philadelphia. 1873. Pp. 64.

FEW would, we think, dispute Dr. Althaus' position as the foremost scientific worker in electro-therapeutics in the United Kingdom, and much of its recent progress is undoubtedly due to his persevering advocacy of its just claims to recognition. Fifteen years have now passed since the first edition of his treatise appeared, and three years since the second edition. The speedy demand for a third edition of a large work, which is the most complete treatise on the subject we know of, is a gratifying proof to the author of the estimation in which his work is held by those best qualified to judge; for it is not one of those pseudo-scientific publications which, in the case of electricity in particular, court the good opinion of the public, and of the public alone.

In one respect only we venture to suggest that Dr. Althaus might still further improve his excellent book, and at the same time escape some hostile criticism—namely, by using the pruning scissors still more freely than he has done, and eliminating all problematical and irrational applications of electricity to practice.

Nothing is so damaging to a therapeutical method as to strain it beyond its rightful limits, and, in electro-therapeutics, no cause has contributed more powerfully than this to create and maintain prejudices which impede its true progress.

Dr. Althaus' book is thoroughly revised, fully illustrated, and well brought out, and we cordially recommend every practitioner to give it a place in his library as the best work of reference on the important subject of which it treats.

Dr. Reynolds' little volume is one of those useful and essentially English books which aim at giving, in a concise and readable form, the writer's matured experience of the practical uses of a therapeutic means. First published in the *Lancet*, and then revised and brought out in a separate issue, these lectures attracted, and deservedly so, a considerable share of attention, and evidently supplied a real want. For the second edition, which has been again subjected to revision, there will, no doubt, be found a large and increasing circle of readers who will profit by the unbiassed and reliable account which Dr. Reynolds gives them of the definite uses of electricity.

Drs. Rockwell and Beard have already evinced their study of the subject by the elaborate treatise on the medical and surgical uses of electricity, which they published some three years since. In the present issue they give their extended experience of the application of electrolysis to the treatment of various kinds of tumour, the more striking effects being obtained with nævus, goitre, and epithelioma. One of the most commendable points in the book is that the authors do not hide their unsuccessful cases; and, although the results claimed in Chapter II. in the treatment of certain diseases of the skin can hardly be allowed to be very convincing, we think that these writers have done well in placing their experience, such as it stands, before the profession.

So far, with few exceptions, we do not think that the American press has contributed much that is solid to our knowledge of electro-therapeutics, and the present publication is one of those valueless productions which only discredit the subject they profess to expound.

The author's syntax and orthography do not argue well for his early education, and, in short, we fail to find in the pages of this book any sufficient reason for its appearance.



*Manual of Chemical Analysis as applied to the Examination of Medicinal Chemicals.* By F. HOFFMANN, Ph.D., Pharmaceutist. New York: Appleton & Co. 1873. Pp. 393.

EVERY pharmacist and dispensing practitioner undoubtedly lies under the responsibility of being able, when necessary, to examine medicinal chemicals as to their identity, quality, and degree of purity. Dr. Hoffmann supplies them with the means of doing this in the guise of a well-executed and well-written *practical guide* in a moderate compass, and we cordially recommend the volume as a judicious compilation, and excellently suited for its intended purpose. An alphabetical arrangement is followed; no formulæ are used, a step which the author scarcely justifies, and the directions given for testing are plain and precise, and in many cases are made quantitative.

The first sixty pages are occupied by a few notes on chemical operations and re-agents, and on some important general tests. Then follows an outline sketch of systematic qualitative analysis, rather too scanty to be of much use; and a capital introduction to that most valuable branch of chemistry—volumetric analysis—is added.

The remaining and most useful portion of the work is devoted to the detailed examination of chemical drugs, from “Acetum” to “Zincum;” and, on the whole, we have not seen a more serviceable companion to pharmaceutical chemists and students than this manual, which is a creditable index of the state of pharmacy in New York.

---

#### WORKS ON DISEASES OF THE SKIN.

1. *Atlas of Portraits of Diseases of the Skin.* New Sydenham Society.
2. *On Lupus-disease of the Skin, and its Treatment by a New Method.* By BALMANNO SQUIRE, M.B. London. 1874. Pp 31.

WE have received the last fasciculus of this splendid collection of drawings, of which no further praise is needed than to say that they are executed with the same artistic skill and fidelity to nature which have characterised the whole series. The illustrations before

us comprise:—*Tinea circinata*, numerous patches on the pubes, thighs, and axilla; *Rupia-psoriasis*, from inherited syphilis; and *Prurigo adolescentium*, a portrait well worth comparing with No. 22, which represents that frequent plague of dispensary practice. viz., *prurigo e pediculis*.

The announcement of a panacea for lupus is almost as great a therapeutical treat as the discovery of a cure for phthisis, yet Mr. Squire believes that he has at length been the fortunate discoverer of the secret, and, like an honest man, he makes no mystery of it:—

“To state it is a very simple matter; it consists simply in repeatedly painting the affected surface with a solution of iodine in rectified spirit. This mode of treatment, I venture confidently to affirm, is capable of healing even extensive cases of lupus, and those, moreover, of very many years standing, within the space of a few months, and that without causing, if the remedy be carefully employed, any breach of surface, much less any loss of substance.”

We cannot imagine why Mr. Squire should be at the pains to write a pamphlet on the merits of an application sufficiently obvious in an affection of strumous relationship, while it cannot even be said that it has been overlooked by others, for if Mr. Squire will take the trouble to refer to the last edition of Erasmus Wilson's large treatise, he will find the iodine treatment for lupus alluded to at p. 429 of that work.

---

*A Handbook of Medical Electricity.* By HERBERT TIBBITS, M.D. With 64 illustrations. London: J. & A. Churchill. 1873. Pp. 149.

As medical superintendent of the National Hospital for the Paralysed and Epileptic, as well as through private practice, Dr. Tibbits has had ample opportunities of studying the application of electricity to medicine, and he has still further identified himself with this subject by undertaking the translation of Duchenne's bulky treatise. In the volume under notice, which aims principally at giving full and explicit details, within convenient limits, *how to use electricity*, we are bound to say that this object is fairly carried out. Electro-physiology is only incidentally alluded to, and the contents are distributed under the headings of electro-medical

instruments, in which a rather undue prominence is afforded to Duchenne's complicated apparatus, the mode of application of electricity, the diagnostic uses of electricity, and electro-therapeutics.

One of the best pieces of advice in the book is the closing paragraph, which the author emphasises by italicising:—

“In conclusion, I would state that the medical practitioner who prescribes electricity should either administer it himself, or cause it to be administered by a skilled operator. The experience of the National Hospital for the Paralysed and Epileptic and my own experience in private practice show conclusively that when patients themselves apply electricity the result has been very unsatisfactory. The most explicit directions will often be misunderstood, or fail in being correctly carried out, the treatment getting undeserved discredit. The rule of practice here laid down is particularly applicable to the localised application of electricity.”

---

*The Effects of High Atmospheric Pressure, including the Caisson Disease.* By ANDREW H. SMITH, M.D., Surgeon to the New York Bridge Company, &c. Brooklyn: Eagle Print. 1873. Quarto, pp. 53.

WERE we to judge of the value of this *brochure* by its length, we should be sadly astray in our estimation. So well-written, concise, and practical a treatise, it has seldom been our lot to peruse, and we can readily understand how it came to be the “Prize Essay of the College of Physicians and Surgeons, New York, 1873.”

The first chapter is devoted to a historical retrospect of the observed effects on the human frame of increased atmospherical pressure. Chapter II. describes the New York caisson, in connection with which the author's observations were made. The object was to throw a bridge, *with a single span of 1,600 feet*, across the river at New York. It was determined to employ the method by compressed air, and two caissons were constructed, having the horizontal dimensions of the proposed piers, which, on the New York side, were 102 by 172 feet. Each caisson was, in effect, a wooden box, turned bottom upward, the interior being 9 feet high. The roof was 22 feet thick, of solid timber, bolted together, and supported by frames running from side to side. Courses of granite blocks were built upon the roof, by which it was sunk until it rested upon

the bed of the river—40 feet beneath the surface. Air was then pumped into the caissons until all the contained water was forced out, and the river-bed left dry. The workmen now proceeded to excavate the earth from beneath the caisson—

“And the vast mass settled day by day down through the gravel and quicksand, which formed the bed of the river, until, at a depth of 78 feet, on the New York side, a solid foundation was reached. In proportion as the caisson settled the masonry upon it was built up, so that the top of the stone-work was always above water. When a solid foundation had been reached, the interior of the caisson was filled with concrete, and the 400,000 cubic feet of timber was left buried nearly 80 feet below the surface, where, practically indestructible, it remains, as the foundation of the tower. Of course,” adds Dr. Smith, “the pressure of air required to keep the water out of the caisson increased in exact proportion to the depth. Thus, at the beginning of the work, the caisson being covered by 40 feet of water, the pressure was about 18 pounds to the square inch, while at the close of the work it stood at 36 pounds.”

The increase of pressure was borne with impunity by the operatives until it reached about 24 pounds, and, after this, cases of what Dr. Smith aptly terms the “Caisson Disease” began to show themselves. In chapter V. he defines it as follows:—

*“A disease depending upon increased atmospheric pressure, but always developed after the pressure is removed. It is characterised by extreme pain in one or more of the extremities, and sometimes in the trunk, and which may or may not be associated with epigastric pain and vomiting. In some cases the pain is accompanied by paralysis, more or less complete, which may be general or local, but is most frequently confined to the lower half of the body. Cerebral symptoms, such as head-ache and vertigo, are sometimes present. The above symptoms are connected, at least in the fatal cases, with congestion of the brain and spinal cord, often resulting in serous or sanguineous effusion, and with congestion of most of the abdominal viscera.”*

The symptoms are described in order—neuralgic pains, the rate of the pulse, condition of the skin, epigastric pain and vomiting, paralysis, and cerebral symptoms. The duration of the disease varies from three or four hours to six or eight days. Death occurs only in cases which are severe from the first—the constant lesion in fatal cases being congestion of the brain or spinal cord. Dr. Smith shows very clearly why, under high atmospheric pressure, the centres will be congested at the expense of the periphery, and how it comes that the *removal of the pressure*, and not the pressure



itself, is the immediate cause of the seizure. "The one essential cause," he observes, "without which the disease can never be developed, is *the transition to the normal atmospheric pressure, after a prolonged sojourn in a highly condensed atmosphere.*" The concurrent causes of the Caisson Disease are—special predisposition, too rapid "locking out," newness to the work, fulness of habit, severe exertion immediately after leaving the caisson, the abuse of alcohol, and entering the caisson hungry. With a view to the prevention of the disease, Dr. Smith prepared a set of printed rules, copies of which were posted up conspicuously at the New York Bridge. Besides this, the treatment is detailed in the clear style so characteristic a feature of the whole essay.

When speaking of the effects of compressed air (Chapter III.), the author records an important original observation respecting the "perspiratory function." Several writers had stated the fact that the secretion from the skin was apparently immensely increased in the caisson. The author's experience, however, goes to prove that in reality the secretion is not materially increased, but that, owing to the dampness of the surrounding atmosphere, evaporation is checked or stopped, the result being an accumulation of moisture upon the surface, which simulated excessive sweating. The clinical notes of 30 cases, which are given, are most instructive, and the suggestions offered in Chapter VII. commend themselves for their simplicity and practical tendency.

*On the Granular Cell found in Ovarian Fluid.* By THOMAS M. DRYSDALE, M.D., of Philadelphia. Extracted from the Transactions of the American Medical Association. Philadelphia: Collins. 1873. 8vo., pp. 8.

THE object of this paper is to describe a peculiar cell which the author has discovered in the fluid of ovarian tumours, which is always present in such fluid, and, as invariably, absent in all abdominal dropsical fluids having an origin other than the ovary; hence he thinks it is "pathognomonic of ovarian disease, and, as such, that its diagnostic value cannot be over-estimated."

We will give the description of this cell in Dr. Drysdale's own words:—

"This granular cell, in ovarian fluid, is generally round, but sometimes a little oval in form, is very delicate, transparent, and contains a number

of fine granules, but no nucleus. The granules have a clear, well-defined outline. These cells differ greatly in size, but their structure is always the same. They may be seen as small as the one five-thousandth of an inch in diameter, and from this to the one two-thousandth of an inch. In some instances I have found them much larger, but the size most commonly met with is about that of a pus cell."

These cells are found mixed with free granular matter, oil globules, cholesterine, epithelium, pus, blood, and compound granule cells; but all these are accidental and may be absent.

"But no matter what other cells may be present or absent, the cell which is almost invariably found in these (ovarian) fluids is the granular cell."

Acetic acid makes the cell more transparent, but makes the granules more distinct. When ether is added the granules become nearly transparent, but the appearance of the cell is not changed.

With regard to this cell we would merely say, that there is, at present, in the minds of pathologists, a deeply-rooted distrust of diagnoses made from the appearances of single cells, and we greatly fear that Dr. Drysdale's granular cell will sink into the same disrepute as that into which the cancer cell and tubercle corpuscle have fallen, even if it should for a time enjoy the celebrity which attended these when they were first described. The characters of the granular cell, as described in this paper, appear to us quite insufficient to establish any diagnosis, and our faith is not increased when we find Dr. Drysdale laying down distinctions between his cell and pus corpuscles, founded on the character of the *cell wall* of the latter—a structure which every tyro in microscopy knows to have no existence, except as a result of the action of reagents.

However, Dr. Drysdale thinks he has made a great discovery, and he gives numerous examples of how in doubtful cases, the presence of ovarian disease was established by finding the granular cell, and ovariectomy performed with the happiest results.

For our own part, we should like stronger evidence on which to found our diagnosis before opening the abdomen.

### PART III.

## MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

### TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

*Wednesday, April 8th, 1874.*

JAMES FOULIS DUNCAN, M.D., President, in the Chair.

*Case of Hæmorrhagic Sarcoma of the Pleura, combined with Extensive Pleuritic Effusion.* By SAMUEL GORDON, M.B., F.K.Q.C.P.; Physician to the House of Industry Hospitals.

THE following case presents many features of interest, not the least of which is, that it elucidates the difficulty of diagnosis which complication of disease causes, whereas either of the morbid conditions existing alone could have presented little or no difficulty.

Margaret Clarke, aged twenty, a servant, from the suburbs of Dublin, reported herself to have been in perfect health, and able to perform her usual work, until five weeks before her admission into hospital. She then got a severe wetting, and was immediately after attacked with pain in her right side and short breathing. The pain gradually diminished, while the difficulty of breathing increased. She was admitted into the Whitworth Hospital on the 20th of February.

She was a strong, well-made girl, and had not the appearance of being affected with any constitutional disease. She had still pain in her right side on deep inspiration. The entire right side was dull on percussion, the dulness extending beyond the anterior mediastinum, and the heart was displaced downwards, and towards the left side. The right side was two inches larger than the left; the intercostal spaces obliterated, the side motionless on inspiration, and she could not lie on the left side. There was no respiratory murmur audible in the right side of the chest, except in the very upper and posterior part, where it was bronchial. The respiratory murmur over the left lung was loudly puerile. The heart's action was very rapid, 120, and feeble. The respiration was 34. The diagnosis given was pleuritis, with very extensive effusion, compressing

the right lung into the cone of the pleura, displacing the anterior mediastinum and the heart. As she had still some pain on deep inspiration, a few leeches were applied to the right side, and she was ordered some mild mercurial. On the 23rd there was no improvement in any respect—on the contrary, the face was rather suffused, and her lips rather blue. I therefore, introduced a small-sized trocar and canula, between the fifth and sixth ribs, and drew off 90 ozs. of reddish serum. After this she was able to lie on her left side. The respiration came down to 28, and the pulse, without diminishing in frequency, became more full. The heart returned almost to its normal position, the bronchial breathing at the apex of the lung was less pronounced; but otherwise the stethoscopic phenomena were not much altered. The secretion of urine was very scanty; contained a large amount of lithates, but no albumen. The grey powder was continued, with a diuretic mixture, containing in each dose a few grains of iodide of potass, and she was allowed a small quantity of wine. On the 7th of March the breathing had again become frequent, and the pressure on the heart so urgent as again to require thoracentesis, and 60 ozs. of the same red serum were withdrawn, with again temporary relief. Again, on the 14th, 50 ozs. were withdrawn, and on the 21st 75 ozs. After each tapping there was decided relief, chiefly evidenced by the effect on the circulating system—the face became always less suffused and the lips of a brighter colour, and the heart invariably rose up to nearly its natural position, and each time that the operation was performed I was led to do so more from the evidence of pressure on the heart, than from the difficulty, or frequency, of respiration. On the occasion of the third tapping I found her almost pulseless, her face becoming livid; I rapidly introduced a trocar, and as the fluid came away she revived, and on this occasion made a very considerable rally for some days.

The internal treatment adopted was quite unsuccessful in producing any of the ends sought for. It was found impossible to mercurialise her, though there was not the least disposition for the mercury to pass away by the bowels: nor did any form of diuretic increase the flow of urine. She rapidly emaciated, had slight cough, with frothy expectoration, which was sometimes tinged with a very light streak of blood. At length her hands became œdematous, her appetite failed, and she rapidly sank. She died on the 30th of March.

The *post-mortem* examination was made by Dr. Yeo, Assistant-Physician to the Hospital, and the following is his account of the recent appearances. [*The morbid parts and a drawing of the recent appearances were presented to the Society.*] Body emaciated and anæmic. Liver considerably displaced—lower margin extending down to the umbilicus, the right half of the diaphragm bulging into the abdominal cavity. Abdominal viscera, and left lung, and pleura, healthy. Heart displaced on the left side. Pericardium



greatly thickened on its mediastinal aspect, blended above with a dense mass of tough elastic tissue, and below with the thickened pleura. It contained about 2 ozs. of clear fluid. The parietal layer of the membrane was also thickened and enlarged on the right side, where were numerous elevated nodules, about the size of a Spanish nut, some of which were extremely congested. On the anterior surface of the heart was a group of very small nodules, similar to those on the parietal portion of the membrane.

The right pleura contained 80 ozs. of the same reddish serum as was drawn off during life. The parietal pleura was greatly thickened throughout, and covered with smooth round knobs, about the size of a walnut, some of which were deep plum colour, and others pale yellowish white. Its mediastinal part reached an inch in thickness, and was found to be continuous with a large mass of tissue which occupied the entire mediastinum. This was found to form an irregular tumour, continuous with the neighbouring serous membranes, and completely enveloped the arch of the aorta and its branches, as well as the cava and innominate veins, and lay around the front and sides of the trachea. The vessels were all quite pervious, and their calibre seemed unaltered. On section this tumour was found to be composed of rounded masses of a soft elastic structure, bound together by a certain amount of ordinary connective tissue. The nodules in the pleura and pericardium were found to have the same properties on section.

The right lung was quite compressed, resembling the tissue of the spleen, quite airless, and covered by a thick, tough layer of structure, similar to the degenerated costal pleura, but more even on the surface. There were not any adhesions at any part of the pleural cavity, and the surface was glistening, and appeared to be covered by a normal serous coat.

Brain not examined.

Microscopic examination showed the greater part of the tumour to be made up of small, spherical, granular cells, of uniform size, connected together by a very small quantity of slender, structureless, intercellular substance—in short, to be a strikingly characteristic example of a *small round-celled sarcoma*. The dark parts were found to be extremely vascular, and in many places studded with small extravasations of blood, which gave these portions of the tumour the appearance of a *hæmorrhagic sarcoma*.

I do not think that it was possible in this case to have diagnosed the existence of the malignant disease up to the time of the first tapping, for the dulness and absence of respiration caused by the pleuritic effusion, joined on so intimately to that caused by the pressure of the tumour, as to leave no intervening space, by which the boundaries of a tumour could be defined. There was no venous congestion, nor enlargement of

superficial veins, the malignant growth (as the *post-mortem* examination showed) being of that soft, yielding nature which allowed veins and arteries to pass through it uncompressed; and, thirdly, on account of the diminished size of the heart and the intervening fluid, there was no intensification of the heart's sounds through the malignant mass.

It becomes another question, whether, after the first tapping, a more correct diagnosis should not have been made. I did, in fact, pronounce that the case was most probably one of secondary pleuritis, for the following reasons:—1st. The colour of the fluid drawn off—it was not serum, tinged with blood, but serum, with which blood was intimately and permanently blended; 2ndly. Because the relief afforded, although considerable, was not in proportion to the amount of fluid drawn off; and, 3rdly. Because there was no corresponding change in the physical signs after the removal of large quantities (on one occasion 60 ozs.) of fluid from the chest.

The diagnosis so far was frequently repeated after subsequent tapplings, and this additional reason assigned that the system refused to respond to any line of treatment adopted.

No medicine which was administered seemed to have any legitimate effect on the system, and at last further paracentesis was declined, on the ground that it was not the amount of fluid in the chest which was causing the patient's death, but some organic disease from which the constitution was suffering.

But the question remains, whether the diagnosis of malignant disease ought not to have been made. The only certain point upon which this diagnosis could have been founded was the nature of the pleuritic fluid. To this point Trousseau has paid most particular attention, and the summary of his observations is that, although the existence of sero-sanguineous fluid is highly diagnostic of the serous membrane being affected with cancer, it does not afford absolute proof of such, and, accordingly, he begins by saying:—"I need not tell you that we possess no positive sign by which to recognize cancer of the pleura, yet if in a woman affected with cancer we find pleural effusion slowly supervening, we may conclude that the bronchial glands and the pleura are themselves the seat of cancerous degeneration, *and the nature of the fluid drawn off will have an especial signification.*" It will be observed, that in the case before us the two first indications were altogether wanting. There was no cancerous development in any other part, and the accumulation of the fluid was very rapid. We had, therefore, only the nature of the fluid to guide us. Influenced by this alone, however, Trousseau pronounced a case operated on by Dr. Barth to be cancer of the pleura, and did so from the recollection of one under his own care several years before. This latter case is so similar to that of Margaret Clarke (except in the existence of an external cancer), that I have ventured to transcribe

it for the information of the Society. On the 9th of November, 1844, a woman, aged fifty-four, suffering from cancerous atrophy of the right breast, became my patient in the Necker Hospital. She had been several months in the St. Louis Hospital for rheumatic pains of the limbs, unaccompanied by any general disturbance of the system. She was having some vapour baths, when, on the 20th of November, returning from the hot room, she felt a chill, and was attacked with acute pleurisy on the right side, which, presenting nothing special in its symptoms, was treated by bleeding, blistering, digitalis, and calomel. About the 20th of December the effusion, far from diminishing, had increased much, and continued increasing until, at the end of December, it had reached the clavicle and supra-spinous fossa of the scapula. By the beginning of January enlargement of the chest was visible. The dulness anteriorly had transgressed the median line, and the heart was displaced a little to the left side. On the 20th of January the dulness had passed four centimetres beyond the median line, and the heart was still more displaced, and the liver could be felt below the false ribs. Notwithstanding this condition, the patient had no dyspnœa; sometimes in the evening slight orthopnœa. There were well-marked fever, puffiness of the face, and œdema of the chest. On the 24th, paracentesis seemed to be absolutely necessary, and was then performed in the usual manner. There were no fits of coughing while the fluid (which was bloody) was escaping. The improvement which followed the operation was but slight. The stethoscopic phenomena were unaltered.

From the 1st to the 11th of February the patient's condition remained stationary; but, on the latter day, erysipelas set in. The effusion increasing, and threatening to suffocate her, I again performed paracentesis, and again drew off sero-sanguineous fluid. The œdema of the chest increased, her strength failed, and she gradually sank. At the autopsy we found the pleura cancerous, and covered throughout with fungoid tumours of encephaloid growth.

It remains only to observe upon the age of the patient and the rapidity of the growth of the disease, and these both may be considered as intimately connected with the form of the carcinoma. Cruveilhier and all subsequent pathologists have observed that this form of malignant disease (of which I have never seen a more characteristic specimen) is that to which the young are most liable,<sup>a</sup> which grows the most rapidly, and is so frequently attended with hæmorrhages; that while they sometimes have actually proved fatal, it is very rarely that a *post-mortem* examination does not afford proof of their having occurred, and on more than one occasion.

<sup>a</sup> In the Richmond Hospital Museum, among other specimens, is one of very extensive hæmorrhagic sarcoma in a child one year old.

The PRESIDENT observed that this being a case of primary cancer, as he understood Dr. Gordon to say, the diagnosis was rendered the more difficult. Did Dr. Gordon know of any family history of cancer?

DR. GORDON replied in the negative. The father and mother of the girl were still alive.

DR. MACSWINEY said that more than twenty-five years ago, when he was acting as the late Mr. O'Ferrall's clinical clerk, he made the *post-mortem* examination of the body of a young girl who died in hospital. She was suffering from pleuritic effusion. He distinctly recollected that Mr. O'Ferrall, who was a man of extremely keen and rapid diagnostic powers, seemed to be possessed of the knowledge that it was not an ordinary pleuritic effusion with which they had to deal; for he made some remarks as to what a mistake it would be if the cavity were opened with a trocar. The girl was not more than fourteen. After death her left pleural cavity was found occupied by a very large mass of cancer, which extended across the chest, from the costal pleural surface to the spine. He rather believed, but was not quite sure, that the case had been fully reported to the Pathological Society by Mr. O'Ferrall. There was in that case no red appearance either in the fluid or in the surface of the tumour, and he was disposed to think that the redness of the cancerous mass itself, in Dr. Gordon's case, was owing to the repeated tapplings. In the case he referred to, one of the grounds on which Mr. O'Ferrall did not treat it as a case of ordinary pleuritic effusion, was the great disproportion between the amount of fluid poured out, and the symptoms of oppression of breathing. For the patient usually, whilst she lived, lay in bed not propped up, was tolerably cheerful, and uncomplaining, and did not seem to suffer any distress in breathing. His impression certainly was that Mr. O'Ferrall did diagnose before the girl's death that it was a case of tumour in the cavity of the chest. There was no external tumour, nor any other lesion to be made out. The mass in the chest was typically cancerous, of the white brain-like variety.

DR. FITZPATRICK said that perhaps Dr. MacSwiney overlooked the fact that paracentesis was hardly thought of in Ireland at the time to which he referred. In the present case the operation was in accordance with the practice of the present day, and was rendered necessary by the action of the fluid pressing on the heart. On one occasion he had under his observation a marked case of what turned out to be one of cerebriform tumour in the posterior mediastinum. The pleura was not involved in that case at all, yet it was a tumour surrounding the trachea, and producing some derangement by pressure on the vessels, with œdema of the face and left side; and the patient died without any pleuritis.



Therefore he thought Dr. Gordon's case most interesting. There was in it evidence of disease of the pleura itself, and they could thus account for the effusion. But there might be cancerous tumours connected with the chest in which the pleura would totally escape, and give no indication of effusion, and yet the patient would die with all the symptoms of exhaustion from cancerous disease. Dr. Gordon's case was one of the most interesting he had heard of for a long time, and it exhibited the progress of improvement in diagnosis, which to him was most gratifying.

DR. MACSWINEY remarked that paracentesis was performed by Trousseau thirty years ago.

The PRESIDENT said the operation performed by Dr. Gordon was perfectly justifiable, for the disease was killing the patient, and the operation prolonged her life.

DR. HENRY KENNEDY agreed entirely with the author, and thought that in this highly interesting case a correct diagnosis could not have been made; the age of the patient, the appearance of health she presented, the absence of hereditary predisposition, and, above all, the symptoms she laboured under being those of an attack of acute disease, all conspired to render the diagnosis next to impossible. In reference to the existence of malignant with acute disease the case seemed to the speaker exactly analogous to those cases where acute disease, like pneumonia, went on, occasionally, to the development of tubercle: a predisposition to a particular termination existing in either case. Speaking of the liquid found, being serum tinged with blood, and on which Trousseau had placed so much weight, he thought it was not so much to be relied on as diagnostic of malignant disease; because the same kind of effusion was, as the author indeed had stated, found in some forms of pleuritis; to which he wished to add certain cases of diffuse inflammation in which an exactly similar effusion existed. So that it was necessary to receive what the French physician had stated with a certain amount of caution. As to the age of the patient, it was well known that the same form of malignant disease might occur much earlier than in the case detailed. He had seen it in the kidney, and read of it in the eye-ball in children, and in one remarkable case which he had seen many years since in St. Vincent's Hospital, it attacked the coats of the small intestines; so as to lessen their calibre very much. The last point to which he wished to revert was in reference to what might be called the secondary occurrence of the disease; that is when a limb or a breast had been removed for the affection, but only to be followed by the same disease within the chest. Under these circumstances it assumed, as far as the speaker knew, exactly the same form which the author had described and exhibited.

DR. HAYDEN once witnessed a case in many particulars like that described by Dr. Gordon. It was that of a young man of twenty-two, who was perfectly healthy till he met with a strain in the chest, and subsequently to this pleuritis was set up. In the course of a very few months the disease ran its course, and the young man died under his (Dr. Hayden's) care in hospital. The diagnosis was not made of malignant disease, but after death the left pleura was found to be filled with a straw-coloured serum. There was some pressure on the vessels of the neck on the left side, but not such as to obstruct them entirely. The heart was pushed to the right side, and one point of great interest was, that the heart in its movement to the right side carried the left lung with it, there having evidently been adhesion of the pericardium to the lung previous to the occurrence of effusion. That was of interest as showing that the pleuritis was antecedent to the malignant development. He had no doubt the first event was pleuritis, and then there being present a latent cancerous diathesis, the disease took a cancerous turn. In that case there was no evidence of cancerous cachexia. The young man was well nourished, and there was no single symptom—with the exception of pressure on the left carotid artery, and universal diffusion of the sounds of the heart over the left side of the chest, which was absolutely dull—which would have led one to suspect the presence of malignant disease.

*Coincident Mitral and Tricuspid Stenosis.* By THOMAS HAYDEN, F.K.Q.C.P.  
Physician to the Mater Misericordiæ Hospital.

I wish to bring under the notice of the Society a specimen of cardiac disease of a rare and exceedingly interesting form. It is a case of contraction of both the auriculo-ventricular orifices, in which the diagnosis of the twofold lesion was made. As far as I can make out by referring to the proceedings of the Pathological Societies of London and Dublin respectively, there are on record only eight cases of this form of disease. Three of these were exhibited at the Pathological Society of London. The first was presented by Dr. Quain in February, 1848. There was tubercular disease of the lungs; no diagnosis of cardiac disease was made, but after death both auriculo-ventricular orifices were found exceedingly contracted. The second case was exhibited by Dr. Pollock in January, 1850. A murmur was heard at the apex of the heart, of systolic rhythm, and Dr. Pollock says that subsequently, shortly before the patient's death, a second murmur of diastolic rhythm was heard over the sternum. In that case both orifices were extremely narrowed. The third case was exhibited by Dr. Pye-Smith in January, 1852. A rough systolic murmur was heard, loudest at the apex, and next a diastolic murmur was heard, but he does not say where. The diagnosis of

tricuspid disease or of mitral narrowing was not made. In the proceedings of the Dublin Pathological Society I find no less than five examples of this form of disease. The first of these was exhibited by Dr. Stokes. The heart was taken from a woman twenty years of age; and here I may remark that of the eight cases I refer to, no less than six were females, and the patients were between twenty and thirty years of age, with only two exceptions. I find that my friend Dr. Foot had had some connexion with Dr. Stokes' case, for his name is mentioned in the record of it. Dr. Stokes stated that there was a pre-systolic murmur, generally predominating at the right side of the heart, and a month before her death it was observed to be pre-systolic, and was audible along the carotid arteries. Thus, there was an approach to an exact diagnosis, for the rhythm of the murmur was detected, and it was localised towards the right side. Death took place suddenly by rupture of an intracranial aneurism. The aortic valves were disorganised. The mitral valves were also diseased, and the mitral orifice greatly contracted. The tricuspid valves were partially coherent, and the diameter of the tricuspid orifice was much reduced, although in a less degree than the mitral.

The next example was exhibited by Dr. James Little in 1867. It was that of a female admitted to the Adelaide Hospital who died rather suddenly. There was no possibility of making a careful examination of the chest; and the consequence was that no special diagnosis of cardiac disease was made, although there was a suspicion that such existed. In that case the double lesion was also found. In February, 1870, a very remarkable specimen was exhibited by Dr. Cryan. The subject was a female aged twenty-five. Dr. Cryan made the diagnosis of mitral obstruction and regurgitation, and also of tricuspid regurgitation, but he did not identify tricuspid contraction. Dr. Cryan made the following remark—and it was of the utmost importance with regard to diagnosis:—"We could distinguish two seats or centres of murmur—one at the left apex of the heart, and the other at the junction of the fifth right costal cartilage with the sternum."

In 1871 I had the honour of submitting the details of a case of this kind to the Pathological Society. It was that of a man twenty-two years of age. In this case there was, in addition to disease of the aortic valves, mitral narrowing and mitral regurgitation, both of which were diagnosed; but, in addition, there was tricuspid narrowing, which was not diagnosed. In reference to that case I made the following remarks:—"I think that, diagnostically, the case is of considerable interest. It is perfectly novel to me, and, with the light the case affords, I should now have no difficulty in diagnosing, in a similar case, the existence of constriction of the two auriculo-ventricular openings. The point on which the diagnosis turns is this, that whereas the murmur of mitral constriction is always at the apex of the heart, and in the great majority

of cases strictly limited to the area of the mitral opening, in this case a murmur of the same rhythm was audible to the left of the sternum. Between these two points there was a portion of the chest over which no murmur was distinctly audible.”<sup>a</sup>

In 1872 Dr. Cryan submitted the particulars of another case, in which, from the presence of systolic apex murmur, and turgescence with systolic pulsation of the jugular veins, the diagnosis was made of “mitral insufficiency, combined, *in all probability*, with contraction of the mitral orifice, tricuspid regurgitation, due probably to dilatation of the right auriculo-ventricular opening; hypertrophy and dilated heart.” Both auriculo-ventricular orifices were much contracted, the left in a greater degree than the right. The heart was globular, and weighed 11 oz.

The case to which I have now to call attention is the following:—On the 11th of last month a female was admitted under my care to the Mater Misericordiæ Hospital. She was twenty-five years of age. The history she gave was as follows:—She had had measles at the age of sixteen, of a very bad kind. During convalescence she caught cold from premature exposure, and she said that from that time forward she was never well. Her breathing became short; she was subject to dizziness in the head and palpitation. Five years ago she spat some blood, and also repeatedly since. Last Christmas she noticed that her female health became deranged, and menstruation finally ceased. Three weeks before her admission to hospital her feet began to swell, and there was a decrease in the renal secretion. The following was her condition on admission:—There was venous congestion of the lips, the lobes of the ears, and of the conjunctivæ; turgescence of the veins of the neck to a remarkable degree; general lividity of the surface; great œdema of the lower extremities; low temperature; oppression of breathing; a constant teasing cough, with little expectoration; the pulse was rapid, and so feeble at the wrist that it was impossible to count it. There were all the signs of œdema of the bases of both lungs, and of liquid in the peritoneum. Precordial dulness was extended towards the right side; the impulse of the heart was strong and rather heaving, and the apex pulsation was to the left of the nipple. At the apex of the heart there was a well-pronounced pre-systolic murmur of the ordinary kind. It was rough, but not remarkably so, and it extended quite up to the first sound. In addition to this there was audible to the left of the sternum, over the sternal end of the fifth costal cartilage, and the corresponding portion of the fourth intercostal space, a murmur of the same rhythm, but much harsher than the former, and extending further backwards into the long pause. Intermediately between the seats of these two

<sup>a</sup> Vol. ii., Part III., p. 256.



murmurs, neither was distinctly audible. This last-mentioned circumstance was of the utmost diagnostic importance, and on this I ventured to make the diagnosis of the double lesion. I repeatedly stated this opinion in presence of the clinical class, and mentioned it to my colleague Dr. Nixon. The woman's case admitted of no hope, and she died on the 14th, the feet and legs having previously become gangrenous. There was some fluid found in the pericardium. The lungs were congested and œdematous. The heart weighed eleven ounces, and was somewhat globular in figure. The right auriculo-ventricular orifice was greatly contracted; it admitted only the point of the middle finger; the tricuspid valves were not very decidedly thickened. The right auricle was greatly dilated and thickened. The right ventricle was not dilated; it contained a good deal of blood clot and decolorised fibrin, which had been entangled in the tricuspid opening. The mitral orifice was still more contracted, barely admitting the point of the index finger. The left auricle was thickened and dilated in a high degree. The orifice of the pulmonary veins was also dilated. The left ventricle was not dilated or hypertrophied; it was remarkably thin at the apex, near the septum.

The point I wish to dwell upon in this case is the existence of two centres of murmur—one of these occupying the usual situation at the apex of the heart, and the other corresponding to the sternal end of the fifth costal cartilage and fourth intercostal space on the left side. Round these points, and within a circle of about two inches and a-half in diameter, the two murmurs were audible, but between them there was a space where neither could be distinctly heard. This constituted the principal element of the differential diagnosis, and it was mainly upon this I proceeded. But the further, though less important circumstance, that the murmurs differed in harshness and in length—the left murmur being less harsh and less prolonged than the right—has to be taken into consideration. This difference I attribute to the entanglement of fibrin in the tricuspid orifice. Finally, there was in this case evidence of general venous obstruction and turgescence; and this (except where copious effusion into both pleural cavities has taken place) is so unusual, in cases of obstruction at the mitral orifice *only*, that I would regard it as warranting a suspicion of the existence of the double lesion. In none of the cases of the twofold lesion that have been reported is the second sound stated to have been either accentuated or doubled—a circumstance of exceptional significance, and to be attributed, in my judgment, to the fact that, as regards obstruction in front, the two ventricles were in a similar condition; the entire range of the pulmonic and systemic circulations respectively being interposed between the ventricles and the seats of obstruction.

DR. MACSWINEY asked was there any, and what was the degree of distinctness of murmur at the junction of the fifth right costal cartilage with the sternum.

DR. HAYDEN replied that there was no murmur audible at the right of the sternum, and the range of diffusion of the right murmur fell short of the right margin of the sternum. He thought it was an error to suppose the area of tricuspid murmur was at all to the right of the mesial line of the sternum. He believed it was to the left. In both the cases he had spoken of, the tricuspid murmur was to the left of the middle of the sternum.

The Society then adjourned.

# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

---

## THIRTY-SIXTH ANNUAL SESSION.

---

*Saturday, 11th April, 1874.*

EVORY KENNEDY, M.D., President, in the Chair.

DR. DARBY exhibited a large mass of hydatids which had been expelled from the uterus of a woman in his hospital on the previous day. She was attacked, at 2 o'clock in the morning, with labour pains, and at 6 o'clock he saw her. There was a good deal of hæmorrhage, and the nurse had plugged the vagina and applied a binder before he arrived. He removed all that was in the vagina, and, feeling convinced that there was more to come (the os being dilated to a size that would admit his two fingers), he again plugged and gave her a drachm of liquor of ergot of rye. At 9 o'clock, pains recurred rather sharply, and he removed the mass of hydatids which was now on the table. The patient was a married woman, and the mother of six children. She had had a miscarriage two years ago. She thought she was pregnant since November last, as the menses due that month did not appear; she became cachectic, looked wretched and weak. He saw her, in consultation with Dr. Whistler, about Christmas; she was then in a bad state of health, and he could feel the uterus above the pubis, and thought it was like a gravid uterus of six months. This was the fourth case (and the third woman) in which he had seen hydatids expelled. Twenty years ago he had under his care a woman who passed an enormous quantity of hydatids. It was the same history as that of the present case. She became unhealthy from the period she dated her supposed pregnancy. She had previously borne two children. She continued in a nervous delicate state of health, with occasional vaginal discharge for ten months afterwards, when she had a similar attack; after this her health improved, and she had had several children since, and is now in good health. He saw a third case, the history of which was pretty much the same. He thought it was a case of pregnancy, but not of healthy pregnancy. There was no appearance of a fœtus in the specimen on the table; there was something about it that seemed to resemble a degenerate placenta, but it was a true case of so-called uterine hydatids.

*On the Use of Perchloride of Iron in Post-partum Hæmorrhage.*

(Adjourned Discussion.)

Dr. MACSWINEY resumed the discussion on the papers of Drs. Atthill and Hill Ringland on the use of perchloride of iron in *post-partum* hæmorrhage. The practice of the injection of the perchloride, he said, had been objected to as being very dangerous; as being uncalled for, inasmuch as the alarming consequences for which it was proposed as a remedy were capable of being met and combated by other means; and, finally, it was said that it was not in all instances successful. It appeared to him that the advocate of this remedy must be in a position to show, firstly, that it was not a dangerous remedy, and that it was efficacious; secondly, that in those dreadful cases of hæmorrhage where the life of the woman was ebbing away, death would in all human probability occur, if the hæmorrhage were not rapidly stopped; and finally it must be shown that there was no other mode of treatment capable of affording speedy and permanent contraction of the uterus. It was for the Society to say how far these propositions were demonstrated by Dr. Atthill or not. In his (Dr. MacSwiney's) opinion he went very far towards proving the position he took up. It was desirable, in a practical point of view, that some satisfactory conclusion should be arrived at, as to the respective value of the liquid, or of the solid preparation of the perchloride of iron. Very satisfactory effects were shown by Dr. Ringland to have followed the application of the solid perchloride to the surface of the uterus, and one of the reasons assigned for preferring it to the solution, was that it obviated the danger of its entrance into the sinuses or the Fallopian tubes. On the other hand, the use of the solid perchloride necessitated the introduction of the hand, and it was a moot point whether it was safe or desirable to introduce the hand into the uterus. The late Dr. Johnson, Dr. Churchill, and Dr. Atthill, disapproved of the introduction of the hand; whilst he gleaned from Dr. M'Clintock's observations that he approved of the practice, which was also sanctioned by Dr. Collins. He (Dr. MacSwiney) had a case when it became necessary to withdraw the placenta, in a case of placenta prævia. Dr. Churchill sanctioned the introduction of the hand in that instance, and the child was delivered. In three days afterwards metritis occurred, and the woman died, and Dr. Churchill observed—"So much for introducing the hand into the uterus." Case number 3 of Dr. Atthill's cases went on well till the 10th day, when the patient had a rigor and died on the 15th day of peritonitis. It might be fairly said that that death should be set down to the use of the perchloride. Another circumstance mentioned by Dr. Atthill was, that all his patients were anæmic persons, and he suggested that their blood was not sufficiently charged with fibrin. It might be a matter of great



importance to ascertain how far Dr. Atthill was right in this view, and he should like to ask whether any member of the Society had ascertained, when dealing with any of these cases, what was the condition of the urine as regards the presence of albumen, carbonate of ammonia, &c. He could quite understand how the blood being in an unhealthy condition would have a greater tendency to exude, than if it were healthy. Cases had been recorded of very severe hæmorrhage where the woman had convulsions with, it might be presumed, albuminous urine before parturition ensued. He never saw a death from *post-partum* hæmorrhage, or from the injection of the perchloride of iron into the uterus. The only case in which he used the perchloride was one of dangerous and profuse menorrhagia—so profuse as to lead to the belief that the lady was about to die from loss of blood. In that case, having to go to the country to see the patient, he took a bottle of the solution with him, and introducing the nozzle of a syringe above the patulous os of an enlarged uterus, he passed in the solution with the effect of arresting the hæmorrhage. In all the cases of *post-partum* hæmorrhage he had seen the patients were of a most weak, lax, and anæmic appearance. In each of these cases he used nitric acid as a test for albumen in the urine, but did not find it. He thought the patients would have died, the hæmorrhage was so great. He did use persevering friction, pressure over the uterus, cold water, and in one case ice and brandy to a large amount. He gave brandy in large quantities repeatedly in one case, and he was sure the lady was under the influence of alcoholic stimulant after she recovered from the danger of death by hæmorrhage.

DR. RINGLAND thought it was the duty of every member of the Society, when such an important discussion was going on, to give the results of any experience he might have had. He would, therefore, make some observations on the question, although he had at first intended to take no part in the discussion on account of his near relationship to the author of one of the papers. Before entering on the subject itself, he should state that he had had occasion in five cases in private practice to employ the perchloride of iron. In three of those cases he used the fluid, and in two the solid perchloride, and had, therefore, had an opportunity of contrasting the action of the two preparations. One of those cases he had had the honour of submitting to the Society, wherein the drug failed to produce the desired effect, and he had then recourse to transfusion, and in that case there was complete recovery. In another case, in which the fluid preparation was used, there was no bad symptom. The third case terminated fatally. In that case, within a few hours after the introduction of the fluid preparation (which was effectual in arresting the hæmorrhage) hysteritis was established, it spread to peritonitis, and the lady was in imminent

danger. After a few days she relapsed into a low fever, and died thirty-one days after her confinement. He believed the employment of the drug was the cause of the fatal effect, but the patient was of a low constitutional habit, thin blood, and had been weakened by considerable hæmorrhage. In the remaining two cases he used the solid perchloride, and in both those cases the recovery was steadily maintained from the commencement. The first question that suggested itself was—Were they justified in employing the drug? To answer this two other questions must first be determined—Was it efficacious for the purpose for which it was employed? and, was it comparatively safe? As to the efficacy of the drug, it appeared that in the 44 or 45 cases which had been submitted to them it failed but in 1 in producing the restraint of the hæmorrhage; and in that case Dr. M'Clintock stated that the non-efficacy of the drug arose from the omission to empty the uterus of the blood, so that the drug was prevented from coming into contact with the uterus. Two other cases were referred to in which transfusion was subsequently employed; but it must be borne in mind that in those cases the restraint of the hæmorrhage was produced, so that, in fact, it failed but in 1 case out of 45. As to the next question—Was it comparatively safe? In Dr. Atthill's practice there had been 1 death; in Dr. Hill Ringland's, 6; in his own, 3; and in Dr. M'Clintock's, 1; making in all 11; but he thought these deaths could not be fairly attributed to the use of the drug, with the exception of one case to which he referred himself, and one case mentioned in Dr. Atthill's paper. But although they might have this considerable mortality in cases of *post-partum* hæmorrhage where the perchloride of iron was used, they must consider how many more deaths they would have had if the drug had not been employed. He could say, as far as his experience in the Coombe Hospital had gone, that a large proportion of those cases would have terminated fatally but for the employment of the drug. As to the comparative value of the solid and fluid preparations, he believed there was a danger of the fluid being absorbed through the sinuses or passing through the Fallopian tubes. He believed the fatal case he had referred to was attributable to one of these causes. The solid drug was free from all danger of this kind, and if there was a certain degree of fatality attending one preparation and not the other, they must give the preference to the method which was unaccompanied by risk. With respect to the introduction of the hand into the womb, it must be remembered that Dr. Hill Ringland mentioned that he had employed every ordinary remedy for the arrest of the hæmorrhage prior to using the drug. Speaking from his own experience, the introduction of the hand was not an unusual practice in the Coombe Hospital; but they knew it was only to be employed after the ordinary means had failed, and in such cases the introduction of the solid perchloride had been attended with success.

There was only one other point on which he wished to say a few words, and that was with respect to Dr. Atthill's view as to vomiting in cases of *post-partum* hæmorrhage. Dr. Atthill appeared to think vomiting one of the most dangerous symptoms that could threaten the patient. His (Dr. Ringland's) experience differed in some respects from that of his friend. He recollected the first case of vomiting following hæmorrhage that occurred in his experience, and remembered the terror that accompanied that event, but he also recollected the valuable effect it had in contracting the uterus. He believed vomiting was a powerful agent in stopping hæmorrhage. This might arise from the vascular sympathy that existed between the uterus and the stomach. He had seen firm contraction of the uterus occur after vomiting, and so strongly did he feel this that he invariably used common mustard as an emetic, and had seen it cause an immediate effect in producing uterine contraction and stopping hæmorrhage.

DR. ATTHILL explained that he had not said vomiting was a dangerous symptom, but that ergot often caused vomiting, and, therefore, could not, in such cases, produce its specific effect.

DR. M'CLINTOCK said he wished to make a remark, by way of explanation. He was afraid, from what had fallen from Dr. MacSwiney, that the remarks he (Dr. M'Clintock) had made on the use of the hand within the uterine cavity might be misapprehended. He did not say the introduction of the hand was free of danger. It was a proceeding he had seldom adopted himself, not more than on two or three occasions in his life. Dr. Charles Johnson, under whom he had acquired a great deal of his obstetric experience, was extremely averse to the introduction of the hand into the uterus after delivery. He had never seen it practised in the Lying-in Hospital when he was an assistant under that gentleman, but he fully admitted it was a most powerful stimulant to the uterus. It produced much pain, and he thought was attended with a good deal of danger, so that it should not, he thought, be resorted to, except under pressing circumstances.

DR. HENRY KENNEDY said he believed, if any bad results followed the use of the drug in the solid form, they would follow immediately. There were a large number of cases in which, though death occurred, the bad symptoms did not appear until some time after the use of the perchloride of iron. It might be a question for discussion whether the drug had anything to do with the fatal result in these cases.

DR. DARBY said the first case he saw of the use of the drug was that of a lady who had miscarried at the third month. She was in a delicate

state of health. He asked Dr. M'Clintock to see her, and that gentleman injected perchloride of iron into the womb, with a successful result. He (Dr. Darby) subsequently introduced the perchloride in two cases of miscarriage, with good effects. He used it only once in *post-partum* hæmorrhage, and it arrested the hæmorrhage immediately. When a practitioner was called on to act in a case of *post-partum* hæmorrhage, it was his duty to do what he thought best. A woman was bleeding to death, and the attendant stopped the hæmorrhage as quickly as he could; and he would not hesitate, from what he had seen, to use the injection. He doubted very much whether absorption into the sinuses of the uterus ever took place. It might possibly produce inflammation, but he doubted that it could get into the Fallopian tubes. He did not think the fatal result in the case referred to by Dr. Ringland was caused by absorption, but would rather attribute it to inflammation caused by the application of the drug to the surface of the uterus. He preferred the use of the fluid preparation, believing that it was more likely to come into contact with the bleeding surface, whereas it was a mere chance whether they could get the solid perchloride into contact with the bleeding vessels.

The PRESIDENT wished it to be distinctly understood that, although, as they were aware, he was not adverse to the introduction of any improved system of treatment that might be applied to the present or any other branch of obstetrics brought before the Society, yet he must warn them that it was very hard to teach an old dog tricks. It appeared to him that one of the chief advantages of having an experienced man in the chair was, that, in case of novelties being introduced, it was for that man to give his opinion honestly and fairly on the merits of any novel practice that might be brought forward; more especially by contrasting its advantages and disadvantages with those confirmed, in his own mind, by his own experience, as well as by the experience of his predecessors and contemporaries. At the same time it was equally his duty to warn them that, whilst on the one hand they were liable to fall into error by hastily grasping at every novelty that might spring up, on the other hand they should bear in mind that the greatest obstacle to the progress of knowledge was scientific incredulity, and the inveterate conviction, on the part of those claiming to be men of experience, that their knowledge could not be improved upon. With this prelude he asked them to take at its value what he was going to say. He could look back for a long time of hospital and of private practice in the treatment of hæmorrhage, and, having retreshed his memory by reference to his notes, he could not bring to mind a case of fatal hæmorrhage in his private practice but one, and that was in the wife of a medical man. He thought, therefore, the alarm which had been got up on the subject as to its frequency, at least in private practice, was unfounded. The President added that he had



little doubt the judicious rules laid down for securing the contraction of the uterus by the late Dr. Joseph Clarke, and insisted upon by Dr. Collins, had much to do with the infrequency of *post-partum* hæmorrhage, and if persisted in would lessen these cases. Be that as it may, it came to this, that whatever system was in practice to check hæmorrhage in his time, must have been such that the risk was not so great as was now supposed, and that it must have been effectual in preventing hæmorrhage. He hailed with pleasure every effort to add to our resources, but he apprehended the result of a rush upon this plan of treatment—a treatment, in his opinion, not unattended with risk, for if every man who met with a dash of hæmorrhage resorted to the use of this styptic, to the neglect of the tried and hitherto successful modes of treatment, the consequences might be most deplorable. It had been stated by Dr. Ringland that out of forty-five cases brought before this Society, treated by perchloride of iron, eleven, or one-fourth, had terminated fatally. He had no hesitation in saying that that was a large proportion of fatalities in hæmorrhage. What were the causes of hæmorrhage after delivery? Imperfect and irregular contraction of the uterus, and morbid adhesions of the placenta to the uterus. The want of contraction was the main cause. In morbid adhesions there was an altered state of the parts that prevented its efficient contraction, at least where the diseased placental structure had adhered; and in the other case there was imperfect action. The object, then, was to cause the vessels to contract, and generally this was effected by grasping the uterus, following down its contraction by pressure, padding, and bandaging, and by not too rapidly extracting the placenta where this is still retained. In 99 out of 100 cases, this proceeding would be found effectual. But where the circular fibres contracted, and there was a chamber locked up above, how could any styptic applied prevent the hæmorrhage. It was possible to account for the efficacy of the styptic where there were altered structures from morbid adhesions, or where there was anæmic inaction. The nature of each case would, perhaps, eventually determine the treatment. A great deal had been said as to the risk consequent on the introduction of the hand. He agreed with Dr. M'Clintock in thinking the practice not justifiable except the conclusion was arrived at that the hæmorrhage could not be otherwise controlled. In such a case it was clearly justifiable, and there was no difficulty about it. He had been obliged in some instances to keep his hand in the uterus for 20 or even 40 minutes, and he believed if he had withdrawn his hand life would have been lost. If they had a case of hour-glass contraction to deal with before or after the expulsion of the placenta, the proper course was to introduce the hand, overcome the spasm of the circular fibres cautiously, thus dilate the contracted portion so as to admit of the contraction of the longitudinal fibres, and remove the clots when the uterus would contract, gradually closing

upon the hand. When they came to decide between the use of the hand and an irritating stimulant thrown into the uterus, it should be recollected that the perchloride continued to act as an irritant whilst it remained in the womb. The hand, on the contrary, was withdrawn, and the irritant removed, after effecting the object for which it was introduced; and why should the hand, when used with caution, cause more injury than the head or breach of a fœtus—the natural stimulant inducing uterine contraction. He had not much experience of the perchloride in *post-partum* hæmorrhage, but he had been repeatedly obliged to use it in other cases of uterine hæmorrhage. He did not look upon it as an innocuous application, and he warned them of this, as he had traced more than two or three deaths as occurring even in minor operations after its use. He stated that distinctly. Taking into consideration the chance of pyæmia, he had no hesitation in saying that his experience went to prove that the use of this application to the interior of the uterus added to the risk in uterine operations, and that the fatality was increased in them by its use. He would not, therefore, have recourse to it unless it was urgently required, believing that other modes of treatment were safer, and, generally speaking, more effectual; but he admitted that cases might occur in which ordinary modes of treatment would prove unavailing, and in such cases he would grasp at the perchloride, and have no hesitation in using it as an additional or *dernier ressort*.

DR. ATTHILL said he was glad he had brought before the Society the question of the use of perchloride of iron in *post-partum* hæmorrhage. He was induced to do so, because he thought that the Dublin Obstetrical Society ought to have an opportunity of discussing this mode of treatment, which at present occupied so largely the attention of the medical mind of Great Britain. Doubtless nothing had been elicited in the course of the discussion either novel or startling, but a body of facts had been stated which he looked upon as of great value, and he believed that the Society, and the medical public outside the Society, would, from those facts, be able to form a very fair opinion as to the value of this mode of treatment. He hoped no one would suppose that it was meant to supplant other modes of treatment by this one; it was simply an addition to the previously practised methods, and it was one that should not, as a rule, be had recourse to until a fair trial had been given to other methods; but cases did arise where the symptoms were so urgent that time could not with safety be expended in having recourse to ordinary modes of treatment—such cases as the first and last of his series, where the patients would probably die in a few minutes from *post-partum* hæmorrhage, if he had not resorted to the use of perchloride of iron. Then, with regard to the mortality which had occurred where the perchloride was used, there had been one death in his

own private practice; but he thought if they were to analyse the results of the cases in which severe *post-partum* hæmorrhage had occurred, it would be found that, though the patients seldom died at the time, the great loss of blood was frequently followed by death at a not very remote date. He pointed out that Dr. Ringland's summary of the fatal cases left an unfairly unfavourable impression on the Society. He gave a mortality of eleven, out of forty-five, or one-fourth, but he did not state as clearly as he should have done that, of these eleven deaths, a considerable number died of diseases in no way attributable to the use of the perchloride of iron. Several of them were far advanced in consumption, and every one engaged in the practice of midwifery knew that phthisical cases went through their labour very well, but afterwards rapidly declined. These cases ought to be eliminated from Dr. Ringland's list. He mentioned other cases where the patient died a few hours after the perchloride of iron had been injected. The probability was that they would have died a couple of hours sooner if it had not been used. The fatal case in his (Dr. Atthill's) practice to which he had referred was a case of fearful hæmorrhage, yet the patient went on well for nine days, and did not die till the fifteenth day. How the perchloride of iron could have caused the fatal result in that case he could not understand. His own view of the action of the perchloride of iron was, that if it did harm, it must do so rapidly. In all cases where fatal results followed from the injection of the perchloride, for the cure of nævi, the patient died rapidly. If he injected perchloride of iron into the uterus of a patient, and that she died suddenly, he would say he had caused her death; but if some time elapsed between the application of the drug and the fatal result, he could not understand why the death should be set down to the iron, for it was an antiseptic. The President had not been quite clear as to the cases in which iron should be used. He gave three causes of *post-partum* hæmorrhage—relaxation of the uterus, hour-glass contraction, and morbid adhesions. The case of morbid adhesions might be excluded altogether, for the iron should never be used until the placenta had been expelled. Hour-glass contraction should also be excluded from the class of cases suitable for the application of the perchloride. In these cases the introduction of the hand would be the proper practice. The case where the uterus was relaxed was that in which the perchloride of iron was useful. In the case of relaxed uterus the introduction of the hand was sometimes very efficacious, but it was not a perfectly harmless practice, and it ought not to be had recourse to until other means had been first tried. There was a certain class of cases in which the introduction of the hand was efficacious and easy of accomplishment—cases where there were a relaxed uterus, a relaxed os, and a relaxed vagina. In such cases if the hand was introduced and the clots cleared out, contraction took place; but sometimes it was found that the uterus subsequently

became again relaxed, and filled with blood, and it became necessary again to introduce the hand. He had heard of the hand being introduced seven times; for his part, he disapproved of such a practice as that. But there were cases where the uterus did not relax sufficiently to allow of the easy introduction of the hand. In the fatal case he had related there was a continuous small stream of blood, but the uterus was never very large, and the os was not sufficiently relaxed to enable him to pass his hand into the cavity; he thought that the perchloride ought to have been used in that case, and he believed had he used it his patient would be alive now. The President showed that the hand was but a momentary stimulant, and in many cases this momentary stimulant having been removed, the hæmorrhage recommenced, and if perchloride were not used the patient would bleed to death. He (Dr. Atthill) was in favour of using the solution in preference to the solid perchloride. It removed the necessity of the introduction of the hand into the uterus, which ought not to be done if it could be avoided, and he believed it was more easy of employment than the solid preparation. It was no easy thing to introduce the hand into the uterus, and to feel one's way round its walls, and he believed this difficulty would be increased if he had a lump of perchloride of iron in his hand. On the other hand, the introduction of the tube of the syringe could be effected under any circumstances, and the solution applied with ease to the bleeding surfaces. An objection urged against the solution was the danger of its passing through the Fallopian tubes, but he considered this danger was very remote; indeed, he disbelieved in it altogether. Having dwelt on the point that the passage of the solution through the Fallopian tubes was a most unlikely occurrence, Dr. Atthill observed that the cases in which perchloride of iron was useful were those of anæmic patients, in whom the blood did not properly coagulate, and when, though the uterus was contracted, the hæmorrhage still went on. Here the perchloride would be useful in sealing up the vessels, and also in other cases where there had been extreme hæmorrhage it would be useful in stopping at once the further loss of blood. In reply to Dr. MacSwiney, he had to say that he never examined the urine in these cases, and that he looked upon the application of vinegar as perfectly useless.

DR. HILL RINGLAND said that at that late hour very few words would suffice as a summing-up on his part. Much had been said, and the points in the various papers had been fully and ably discussed by the several speakers. Dr. Atthill seemed to think the introduction of the solid salt a matter of great, if not of insuperable difficulty. All he (Dr. Ringland) could say was, that in the twenty-three cases wherein he used it, the operation was always effected with the greatest ease and certainty. To use the injection, the salt, in a fluid form, was needed, and to introduce the injection a syringe was a necessity. More than once had he



seen the latter burst just at the most critical moment, an accident much to be dreaded and deplored ; but in the solid application of the drug, the only instrument required was that with which nature had supplied them all—the hand. Dr. Atthill was correct in stating that the rate of mortality had been exaggerated. In reply to the President's observations, he should wish it to be understood that all the cases detailed by him occurred in the extern practice of the Coombe Hospital, and that many of them, if not all, were literally beyond hope before seen ; blood was pouring from them ; means of an ordinary kind were at once employed, but resulted in failure ; death was inevitably rapidly approaching ; iron was then used, and in all, save six, a favourable termination ensued ; and even in those six cases the diseased pathological conditions were more than enough to cause the fatal result. The drug was used as a *dernier resort*, but dangerous cases demanded dangerous remedies.

The Society then adjourned.

## PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

DR. LYONS, President.

DR. BENNETT, Secretary.

*Laryngeal Phthisis*.—DR. WILLIAM MOORE said the morbid specimen he exhibited was taken from the body of a female servant, twenty-eight years of age, who was admitted to Sir Patrick Dun's Hospital early in November last. The most remarkable feature in the case was, that on admission she had almost complete aphonia. It was nearly as complete aphonia as he had ever heard—the patient could speak only in the merest whisper; occasionally there was a slight attempt at intonation, but only an attempt, which eventually disappeared. The patient stated that the aphonia had been in existence for about six weeks before her admission, and that latterly she got so feeble she was obliged to come to hospital. She had also a slight dry cough, but she complained of no affection of the chest. She suffered from dyspnœa, complete loss of voice, and a kind of whistling or hissing respiration. On examination some evidence was found of consolidation of the top of the right lung, but there were no very remarkable or special signs of pulmonary phthisis, although she was much emaciated. Her great distress was the dyspnœa, and the hissing or stridulous kind of respiration. The patient seemed to improve under treatment, till the 18th December, on which night he was sent for to see her. She was gasping for breath, and was in a most distressing condition. He examined both lungs, and found little or no air entering either lung. Under the use of hot fomentations to the chest and warm inhalations, the distressing symptoms abated and she got some relief, but the attack returned on two or three occasions, till, at one o'clock in the morning of Christmas day, she awoke, gave a severe gasp, and died very quietly. It might be said that this was the history of a great many cases of pulmonary phthisis, which this proved to be; but he brought the case forward for the following points of interest:—In the first place there was some tubercular deposition in the lungs, but when they came to examine the trachea, there was œdema of the epiglottis and of the epiglottidean folds, the œdema of the latter, especially, being very great. Upon opening the larynx, they found tubercular deposit at the base of the epiglottis, ulceration of the arytenoid cartilages, and detachment, from ulceration, of the true vocal chords from the arytenoid cartilages. There was evidence of ulceration in the cricoid cartilage in two or three spots, and, on coming

further down, there was sub-acute inflammation of the trachea. His object in drawing the attention of the Society to the case was to show the pathological value of the symptoms of aphonia. This was as *complete* a case of aphonia as had ever come under his observation. It differed from the aphonia seen in connexion with pharyngeal and laryngeal syphilitic affections, which were characterised more by hoarseness than by aphonia. It differed from the aphonia in connexion with aneurism, in being more complete than that usually found in connexion with pressure on the recurrent nerve, and in the fact that from the time this woman entered the hospital till the day of her death, it *persisted*, whilst aneurismal aphonia is apt to *remit*. This persistency is explained by the detachment of the true vocal chords from the arytenoid cartilages, they being, in fact, completely cut off by the ulceration, and being thickened and well nigh obliterated by the tuberculous deposit; whilst the hissing, whistling inspiration can be explained by ulceration, not only of the arytenoid cartilages, but also of the aryteno-epiglottidean folds.—*January 10, 1874.*

*Abscess of the Liver.*—DR. GRIMSHAW said the viscera which he exhibited had been taken from the body of a woman who died in Cork-street Fever Hospital. She was admitted on the 27th of November, and died on the 13th of January. She was aged thirty-seven, a widow, and had three children. She was labouring under what he believed at the time, and still believed, to be enteric fever. At the same time she said she had been delicate for some time previously, although she dated her febrile illness only five days before her admission. He found she had evidence of chronic phthisis and cavities in both lungs. She got over the febrile state, which passed through the ordinary stages of a mild attack of enteric fever, and she had all the characteristics of the disease, except spots. At the time he expected convalescence to be almost complete she appeared to be making no progress towards recovery. She remained in the same condition for some time, even when her temperature had fallen down to the normal standard. He thought that by treating her with tonics he could get her into a better condition. The cough did not get worse, and the lung disease did not appear to be making any advance. On the 1st of the present month she got an attack of shivering, followed by hot and sweating stages. She said she had previously suffered from similar attacks, like *ague*. That was repeated on the next night, and the night following, and he believed on four successive nights. Having examined the abdomen, he discovered an enlargement in the left hypochondriac region. She had lived all her life in the Liberties of Dublin, which was not a likely place for a malarious fever to originate. He came to the conclusion that there was an enlarged spleen, and treated her with

anticipatory doses of quinine, which prevented the recurrence of the aguish fits. The tumour increased in size. He could discover no fluctuation in it. However, she gradually got worse, complained of pain in the region of the tumour, and died. He attached considerable interest to the result of the *post-mortem* examination, and examined all the organs carefully. The heart was perfectly healthy, but smaller than natural. The lungs exhibited the condition he anticipated—large cavities in both; the pleura was adherent in several places. On opening the abdomen he found that all the parts in the vicinity of the tumour were glued together by peritoneal inflammation; the adhesions were not very strong, and only towards the left side of the abdomen, the right side being perfectly free. He found that the kidneys and spleen were perfectly healthy. In removing the viscera of the chest and abdomen, he had left the diaphragm, liver, stomach, and some portion of the intestine involved in the mass. He removed the diaphragm and the liver together. He found at one point a portion of omentum adhering to the tumour; from an opening in this pus was issuing. This opening was connected with an enormous cavity in the left lobe of the liver. It occupied the whole of the left lobe, with the exception of a small portion. The roof of the abscess was formed by the diaphragm itself. There was scarcely any liver tissue of the left lobe remaining; a small portion only of the upper part of the left lobe was tolerably healthy, and it was much congested. The stomach he found healthy, but attenuated, as it usually is in cases of prolonged disease. She had profuse attacks of vomiting during the latter weeks of her life, which could not be controlled by any means. The right lobe of the liver was somewhat enlarged, but in making a section from right to left there was no appearance of disease, except in that portion bordering on the abscess; there were some small points on the surface, indicating former peritonitis, with adhesions. The large abscess contained a pint of fluid. Half a pint of it was collected, and at least an equal quantity was lost. The case was interesting, because of the rareness of the situation of the abscess, and next, because there was no assignable cause for it. It was not of the nature of apyæmic abscess; it appeared not to have any but accidental connexions with neighbouring organs. There might have been an injury, but there was no history or trace of such. He examined the intestines carefully. There were some parts where there were doubtful appearances of previous ulceration, but he did not find anything to substantiate his belief that the case was one of enteric fever; and yet all the symptoms pointed to that. She had diarrhœa with ochreous stools, pain in the iliac fossa, and the range of temperature was that usually found in enteric fever. The most important point, however, was the error he fell into, from the general aguish symptoms, of mistaking this enormous abscess for an enlarged spleen.—*January 17, 1874.*



*Chronic Renal Disease.*—DR. HAYDEN exhibited what he thought was a good example of the large white kidney. The organs were not remarkable for their size, but in texture they were good examples of the disease. They were obtained from a boy, twenty years of age, admitted to hospital on the 5th of January, and who died on the 12th. He had been a frequenter of the hospital for the previous three years, and Dr. Hayden had had ample opportunities of following the details of his case for that period. He was a car-driver, and accustomed to indulge in dissipation occasionally. He had been present for many years past at the great annual event at Punchestown, and after the race he was always a patient of his in the hospital. On those occasions he caught cold, and his symptoms were those of bronchitis supervening on emphysema of the lungs in an aggravated form, and accompanied by congestion of the kidneys and anasarca. He passed comparatively little urine, and that was loaded with amorphous lithates, but contained no albumen. He was on one occasion attacked with uræmic convulsions of a formidable kind, and was unconscious for about twenty-four hours. Dr. Hayden saw the man on the 4th of this month driving his car, and on passing him made the reflection that he was likely to have charge of him soon again, and on the following day he found him occupying one of his beds in the hospital. His condition was nearly the same as on former occasions, but one half the volume of the urine now consisted of albumen, the specific gravity being only 1,011. He went on in the ordinary way, and on the 10th was again attacked with uræmic convulsions of the most formidable character. For twenty-four hours he was in a state of coma, with a succession of convulsive seizures. On the 11th, however, he was conscious, and able to answer questions. He described himself as suffering from a feeling of tension in the head; he had scarcely any pulse, and over the entire surface of the body there was capillary congestion. He was again attacked with convulsions that evening, and died in the course of the night in a convulsive paroxysm. The diagnosis was a very simple matter—emphysema of the lung, with recurrent bronchitis, dilated right chamber of the heart, chronic renal congestion, and on the last occasion organic renal disease. The lungs were remarkably voluminous and emphysematous. The heart was large, and weighed seventeen ounces. The right ventricle was remarkably dilated, and the walls thickened. The left was hypertrophied and dilated. There was no valvular or arterial disease. The tissue of the heart was strictly sound. The kidneys formed a good example of the smooth white kidney. The capsule peeled off readily; the cortex was thickened considerably. It presented the usual appearance under the microscope—viz., thickened and dilated tubules, distended with epithelium and granular *débris*, mixed with oil globules, and a few red blood corpuscles.

The kidneys weighed four and a half and five ounces respectively. It

would seem to him that they were in the state of transition from hypertrophy to atrophy. It was certain that within a period of seven months this disease had set in and run its course in the kidneys, for in May last this man was under his care. The urine was then carefully examined, and there was no evidence of renal disease, other than vascular congestion.—  
*January 17, 1874.*

*Cirrhosis of the Lung.*—DR. HUGHES said the specimen which he had the honour to exhibit was taken from the body of a patient who died in the Mater Misericordiæ Hospital on the 27th December. She was a woman thirty years of age, and had been an invalid for the last three years. The *post-mortem* examination was made the day after she died by his colleague, Dr. Nixon, in his presence, assisted by the resident pupils, Messrs. Duff and Dempsey.

The body was much emaciated. On raising the sternum, the right lung was seen to occupy more than its normal portion of the thoracic cavity, and extended beyond the mediastinum to the left.

On the left side there was nothing visible but the pericardium, containing the heart, which lay high up underneath the second and third ribs, where it was heard and felt during life. The lung lay behind it with its pleural covering adherent throughout to the costal layer and to the diaphragm.

The removal of the thoracic organs was difficult, owing to the firmness of the adhesions, especially on the left side. But there was no effusion in either pleura, or into the pericardium.

In the abdominal cavity the liver alone was visible, occupying its entire space, and pushing the intestines downwards and backwards. There was no effusion here either, but some insignificant traces of peritonitis.

When both lungs were removed, it was found that the *right* one was at least three times as large as the *left*. It was not at all so adherent as the other; it was filled with air and floated in water, while the *left* was contracted to almost the size of the closed hand, invested by a false membrane, so adherent in some parts that it could not be separated from the lung. When cut into, many small caverns were laid open, varying in size from a small pea to a hazel nut, lined by a smooth membrane, and all continuous with the bronchial tubes, which were much dilated. These cavities were very general throughout the entire lung, but more especially so towards its periphery. The lung tissue around the caverns was exceedingly dense, tough, and inelastic, not yielding to pressure, and containing neither air nor fluid. It was traversed by numerous whitish bands, crossing each other in every direction. It could not be inflated, and sank in water. The right lung was also closely invested by a thick, tough membrane, which, however, could be separated from it, except at the base, where it was intimately adherent to the diaphragm.

On making a section through this lung, the greater portion of it seemed healthy, but congested, and of a dark mahogany-colour. It was crepitant on pressure, and floated in water. At the apex, however, a considerable cavern was opened, lined with a perfectly smooth membrane, and containing a secretion similar to that in the caverns of the other lung. Scattered very thinly throughout both lungs, but more notably the left, were isolated collections of a caseous deposit, of a whitish colour, not larger than a grain of shot, of a soft consistence, except in instances where it was hard as grit, when picked out of the pulmonary tissue.

In the abdomen the liver was very much enlarged, weighing 8lbs. 2ozs. It was rather pale in colour, perfectly smooth, and quite hard. It was in a state of waxy or amyloid degeneration, and readily yielded the characteristic colour, when the iodine test was applied. The kidneys had also undergone the same process of degeneration. The spleen was apparently normal, as was also the heart. The head was not examined.

From the time this patient was admitted into hospital until she died, a period of only nine days, she was in a very weak and exhausted condition, having suffered from an attack of hæmoptysis immediately before admission, and from nausea and vomiting for some days after. She lived, as already stated, only nine days in hospital; and when relieved of the gastric symptoms, she lapsed into a state of stupor from which she could with difficulty be aroused until her death on the 26th ult.

Owing to these circumstances, it was scarcely possible to obtain any account of her previous symptoms and history. But the kindness of Dr. Foot had supplied the desired particulars in the following note:—

“DEAR DR. HUGHES,—I find from my note-book that Mary Tighe was thirty years of age in March, 1873. She had then been affected in the chest for three years. The first hæmoptysis was in 1871—streaky hæmorrhagia two or three times a week, violent cough, and profuse expectoration—the latter coming up in gulps; it never was fetid, but often very salty. The apex beat of heart was, in March, 1873, in second left inter-costal space, and she was aware of its having been there for a long time. She had much flattening of left chest anteriorly, prominence of right; tubular respiration, and gurgling rales over left summit before and behind, with pectoriloquy and great dulness; while the opposite lung had puerile respiration, and was hypertrophied, crossing to left of sternum. Amenorrhœa had long existed.

“When admitted into the Meath Hospital for the fourth time, in November, 1873, she had ascites, and enlargement of liver to four and a-half inches below ribs—smooth, but tender—marked increase of dyspeptic symptoms, and an intermitting aphonia, considered due to pressure on left recurrent laryngeal nerve. Thick yellow expectoration came up without effort, and was very salty. She had œdema of legs and feet before last admission. On this occasion she had hectic fever, not observed when in hospital before. The diagnosis made at first visit to hospital was cirrhosis and bronchiectasis of left lung. I was informed she was of very intemperate habits. I am aware she possessed a very violent and unmanageable temper. She left hospital on last occasion because she was not getting well.

“Very faithfully yours,

“A. W. FOOT.”

When she came to the Mater Misericordiæ Hospital, six weeks later, and for the first time, her case resembled in many respects one of tubercular phthisis. But, although there was a history of cough, hæmoptysis and wasting, with a dull side on percussion, together with gurgling and pectoriloquy, still there was no very marked hollowness of the cheeks, or hectic blush; no quickened pulse (it was only 70); no sweating; no diarrhœa; and she had no expectoration while in hospital. When this woman was a patient under Dr. Foot's care, he diagnosed her case as one of cirrhosis and bronchiectasis. Indeed, the patient herself said so, and the *post-mortem* appearances entirely confirmed the accuracy of that diagnosis. A case of this rare form of pulmonary disease had been already recorded by Dr. Foot, and, as is well known, was first recognized and described by one of the most distinguished members of the Society, Sir D. Corrigan, with that lucid simplicity, unerring fidelity, and completeness, so characteristic of his contributions to medical science. Later observers had also described this affection, under the varied designations of "chronic pneumonia," "interstitial pneumonia," and "fibroid phthisis;" but they had not added a single fact to Sir D. Corrigan's original observations. For, no matter how writers may differ as to a name, the essence of the disease appeared to consist of a contractile action taking place in the cellular and fibro-cellular tissue of the lung, which first obliterates the air vesicles, and, in its progress of contraction, draws in from the circumference towards the centre the yielding lung tissue, and so diminishes the size of the organ, and also assists in producing dilatation of the bronchial tubes, as Sir D. Corrigan had described in his original paper in the *Dublin Quarterly Journal*. One thing, at all events, was obvious. The specimen was not like an ordinary hepatised lung, which is not diminished in volume when it becomes solid, which is friable and easily breaks down under pressure, and which exudes fluid when cut into. On the contrary, the cirrhotic lung is tough, contracted, and does not contain any fluid. It is not a lung compressed by pleuritic effusion, for there was not, nor could there have been, any fluid in this universally adherent pleura, nor could it, unlike the simply compressed one, be re-inflated by any effort.—*January 17, 1874.*

DR. TYRRELL said the specimen he had the honour to exhibit was one of extreme rarity—indeed, he believed it unique. It was an example of spindle-celled sarcoma of the testicle, in which a portion of the omentum was attached to the testicle, and not discovered before operation. The patient was a farmer, aged forty years, and in June, 1872, he came to Dublin, and placed himself under Dr. Tyrrell's care. There was then a tumour, about the size of a large orange, on the left side. The history of the case was as follows:—Up to the age of puberty he had no testicle on the left side, but a soft mass in the groin that did not give him any



trouble. When he came to manhood it became larger and passed into the scrotum. He was occasionally able to push it up, but it came down again. He was married at thirty, and his wife had five children. About three weeks previous to his coming up to town it passed down into the scrotum, and he could not get it up again. In Dr. Tyrrell's note-book, in June, 1872, he found it stated that the tumour was smooth on the surface; that the skin moved freely over it; that the cord was thickened but could be felt; that there was no impulse on coughing, and no enlargement of the glands. He tapped the tumour by an aspirateur and drew off about half an ounce of arterial blood. He then put the patient on a course of mercury, and after the medical treatment the tumour did not decrease. The man declined to have it removed, and returned to the country. He came up again in last September, fourteen months afterwards, when the tumour had reached the size depicted in the drawing which he now exhibited.



Sarcoma Testis, with adherent Omentum.

It was evident it was a large tumour, probably of a malignant nature, and his colleagues agreed with him that castration should be performed. They had not, however, the faintest idea that the complication he was about to describe was present. He was surprised to find a

large portion of omentum attached most intimately to the bottom of the testicle. This rather alarmed him. However, under the circumstances, he ligatured the omentum with a cat-gut ligature, put it into the abdomen, and treated the patient as he would after an operation for strangulated hernia, and he got well without one bad symptom. The ligature from the cord came away on the sixteenth day, and the man returned to Tipperary quite well. It was well known that a testicle retained in the abdomen or in the inguinal canal, was more liable to disease than in the normal position. Virchow said this probably arose from its being subjected to more pressure from the abdominal walls than it would sustain in the scrotum. In Curling's book, and the works of other authors, the complications attending these retained testicles were mentioned; but in none of the books he had consulted was there mention made of this special complication; there was no mention made of an omental hernia occurring under such circumstances. This case should make surgeons most cautious in the diagnosis of such tumours, and in the operation for their removal. It had been proposed by some surgeons to apply an ecraseur; and while the principal surgeon was enucleating the testicle from the cord, the assistant would be tightening the chain of the ecraseur. In that case, if there were a small piece of intestine attached to the testicle, there would have been a fatal result. His colleague, Dr. Coppinger, and Dr. Robert Mc'Donnell had examined the tumour, and pronounced it to be a well marked specimen of the spindle-celled sarcoma. If it were of a more malignant nature, such as the encephaloid form, the glands would have been enlarged in the lumbar region.—*January 17, 1874.*

*Enteric Fever.*—DR. STOKES said the patient from whom the specimens were obtained had been a servant, taken from the workhouse, and employed in a gate-lodge. She must have had a severe mistress, for it appeared she had remained out under a bush for certainly three nights. How long she had symptoms of fever while compelled to remain at service did not appear. A friend brought her to the Meath Hospital on the 15th of January, and she died on the 23rd. The case all through had the characteristics of severe enteric fever. She had slight tenderness of the abdomen, a weak pulse, and rapid action of the heart. In this state she continued for several nights. She had had diarrhœa for two days before she was admitted, but this was not caused by purgative medicine, and she had not got anything in the way of food likely to produce it.

One characteristic typhoid spot was seen on the chest on the day after admission, and on two subsequent occasions rose-spots were seen.

She never had much diarrhœa, except on the day before she died. The temperature was always very high, being on one occasion 105·8°; the evening rise was very well marked, and the temperature fell to 100° on the morning of her death. The exhibition of quinine had no effect

in bringing down the temperature. For some days before she died the heart's sounds were inaudible, clearly from their great weakness, and also from the co-existence of an intense bronchitic rale over the chest. On the morning of her death the heart's impulse was jerking, and under a condition of vehement excitement. In the lower portion of the ileum the agminated and solitary glands presented a great amount of acute disease, with tumefaction of the mesenteric glands. Towards the ileo-cæcal valve the ulceration was very extensive and coalescing. The upper lobe of one lung was consolidated and tough, breaking with extreme difficulty. There were spots of consolidation through the left lung. There was nothing remarkable either in the spleen or in the state of the heart. The specimen illustrated the prevailing epidemic characters of disease, and in this respect was very interesting. Discussions had, indeed, arisen from time to time as to the differences between typhoid fever and typhus, but it appeared to him that a more important circumstance would be the points of agreement between these forms of essential fever, and this case, so far as the nervous condition was concerned, might well have been recorded under one or other of these forms of disease. In both these forms there may be essential and there may be local disease; and both the essential and local maladies are equally subject to the law of periodicity. Both typhus and typhoid fever, as far as their local and essential elements are known, are subject to this great law of periodicity. They are also subject to variability of character, and of their constitutional and local symptoms. As to local conditions they both show variation in intensity, in amount, in seat, in combination, in the period of appearance, in cessation, and, in fact, in the general malady. This character of inconstancy applied to both typhus and typhoid. It was also certain that the local symptoms might precede, or be accompanied with organic change, or with merely functional derangement. Local symptoms interfered remarkably with the law of periodicity—that law under which disease ceases spontaneously. In both fevers, too, the local disease, taken in a re-active condition, may act upon the essential malady, so that there is then a symptomatic added to the primary or essential fever.

It was unnecessary to enter into the question of the co-existence of both fevers; but there was little doubt that the physician would occasionally meet with a case which he could not say was typhus without typhoid, or typhoid without typhus. Both forms were more or less contagious, and as to the principles of treatment, he believed they were the same in both—namely, to contend with the effects of the local disease, and support the patient's strength, thus prolonging life, until, by the operation of the wonderful law of periodicity, the disease spontaneously subsided.—*January 24, 1874.*

*Pneumonic and Intestinal Phthisis.*—DR. A. W. FOOT laid before the Society the viscera of a young man, aged twenty-three, who had died in the Meath Hospital. The lungs were laid open in a manner calculated to show the various pathological processes at work in their different portions. The anterior halves of each were removed by an incision carried from above downwards, parallel to the sternum. The substance of the left lung was in the condition described by Laennec as “tubercular infiltration,” or by Addison as “light grey induration;” Dr. Foot, in accordance with the pathology of the present day, would speak of it as caseous pneumonia. A large multilocular vomica occupied the greater portion of the superior lobe of this lung. The upper and middle lobes of the right lung also were indurated, but in a lesser degree, by the infiltration of light grey pneumonic exudation, more recent than that on the left side, and not as yet exhibiting much appearance of softening. The lower lobe of this lung presented one isolated patch of greyish yellow exudation, the size of a shilling; the remainder of the tissue of this lobe was in a state of red hepatisation from a recent attack of pneumonia, which had been the immediate cause of death.

The young man had been one of delicate appearance; a sister of his had died of consumption at twenty-one years of age. He caught a cold from a severe wetting ten months before his death; the subsequent history of this “cold” was that of catarrhal pneumonia. He had been under Dr. Foot’s observation for four months before his death; he never expectorated blood in any form during his illness. Myoidema was very striking, especially over the right side of the chest, from the time when he was first seen. He suffered very constantly from pleurodynia about the region of the heart; he was not troubled with diarrhœa. The *bruit de pot fêlé* was easily elicited by percussion over the left apex. He considered himself sufficiently improved to leave hospital in November, 1873, and attend as an out patient, but was re-admitted early in January, 1874, with an aggravation of all his bad symptoms. The rapidity of the pulse and respiration had increased, the temperature was higher, the debility and night perspirations were more marked, and the physical signs had greatly extended; at this time the only part of the lung in which anything like normal respiration could be heard was the inferior lobe of the right lung. Seven days before his death he “caught fresh cold,” by going out on a harsh day; this cold was an inflammatory attack of the only remaining available portion of lung, the right lower lobe. During the last thirteen days he was in hospital his average temperature (26 observations, made twice daily) was  $102^{\circ}.3$  Fahr.; his average pulse (27 observations) was  $125.3$ ; the average rate of his respiration (27 observations) was  $35.4$  per minute.

The lungs externally presented evidences of former inflammations of their serous covering; the pleuræ, especially on the left side, were



thickened, and united by thinly sheeted layers of exudation; the sides of the inter-lobar fissures were adherent; the connexions to the pericardium, especially on the left side, were close and thick; the bases were attached to the diaphragm. The mucous membrane of the trachea exhibited unusual vascularity, increasing towards the bifurcation, where the crimson surface was spotted with patches of ashen-grey exudation, not removable with a current of water; these patches coalesced into sheets towards the left bronchus, which was almost uniformly coated with them. The retro-bronchial and peri-tracheal glands, also many of those in the anterior mediastinum, were enlarged and succulent. The lungs as a whole were large, firm, and stiff, standing up beside and round the heart as if injected with wax; there was, however, one marked concavity externally, where there was a tendency to collapse over the site of the large vomica. The light grey colour of the solidified portions was diversified by scanty, but numerous, depositions of dark pulmonary pigment. There was not a tubercle of a typical form to be seen or felt, on a pretty careful examination, on the organs or membranes of the thorax. On the phrenic peritoneum covering a tendinous portion of the diaphragm, contiguous to the coronary ligament of the liver, a number of tubercles were seen, resembling a spattering of small herpetic vesicles, not quite crystalline, but semi-opaque; they were disposed in small, dotted groups; when the serous membrane was raised off they came with it. Very typical tubercles were discovered with the microscope in this portion of the phrenic peritoneum. There were no tubercles visible to the eye on the surface of the spleen, which was partially adherent to the diaphragm, and weighed 7 ozs.

The solitary "glands" were enlarged over the whole of the small intestinal tract; in the duodenum they presented themselves as round whitish bodies, the size of hemp-seeds, towards the end of the ileum; there were most characteristic ulcerations of both solitary and agminated glands. The groups of cup-shaped excavations in Peyer's patches were very marked; the mesenteric glands were universally enlarged. In three or four places on the serous coat of the ileum, corresponding to the most diseased Peyerian patches, were small hard knots of true tubercles, the bowel here exhibiting intestinal phthisis on its mucous, and intestinal tuberculosis on its serous surface. With the exception of the vermiform appendix, whose numerous glands were profusely affected, there were but two enlarged solitary follicles found in the large intestine.—*January 24, 1874.*

*Excision of the Knee.*—DR. THOMSON said the specimen he exhibited was removed from a female patient in the Richmond Hospital on the preceding Wednesday. She was admitted ten days previously, suffering from a contracted knee. The history she gave was that about six

months previously she had a severe wetting. Two or three days afterwards she had rigors, and then pain in the knee. This was followed by great swelling, and she was obliged to take to her bed, so great was the pain. From the first week she had startings in the joint, and was unable to leave her bed. She lay there five months, without any medical or surgical attendance whatever, and at the end of that time, when she got up, she was unable to extend or to flex the limb. After remaining at home a month she came to Dublin, to see if anything could be done to remedy the deformity, and was admitted to the Richmond Hospital. On examination, it was quite clear that what had taken place was bony ankylosis. It was impossible, by any force, to extend or to flex the limb. The patella was perfectly fixed. She was anxious that something should be done, and the risks of operation having been stated to her, she assented to that course. The operation consisted in the removal of a wedge-shaped piece from the knee corresponding to the angular deformity. In this case the knee was flexed at about right angles. The ordinary U-shaped incision was made, and when he came down upon the bones, it was found that the patella was perfectly adherent to the outer condyles. This had first to be removed. A wedge-shaped portion of bone, extending almost, but not entirely, through the ankylosed mass, was taken away, and, by increasing the flexion, the remaining band of bone was broken through. It was found necessary to remove the whole of the condyles of the femur, in order to get the limb into position. The extremity was then placed on a plain back splint, with a foot-piece, and put up in plaster of Paris. In this case he adopted Esmarch's method of bloodless surgery. Not a single drop of blood was lost, and the knife did not bear any stain upon it. No vessels were secured. The case was one of interest in several respects; among the more prominent points being the rarity of this operation, the rapidity of the destruction of the joint, and of its repair by perfect osseous union. The original disease commenced six months ago. The woman lay in bed five months, and during that time the joint was disorganised and repaired. The whole of the semilunar cartilages had been absorbed, and of the articular cartilages, only a small portion remained upon that part of the internal condyle not in contact with the tibia. There was no sign of disease in the bones.—*January 24, 1874.*

*Bright's Disease, Œdema of the Glottis.*—DR. HAYDEN exhibited the viscera of a man who was admitted to the Mater Misericordiæ Hospital on the 1st of January, with symptoms of bronchitis, with emphysema of the lungs. There was great dyspnœa—not, however, excessive. The ordinary treatment was made use of, and the course of the disease seemed to be normal up to the week before his death, which took place on the 16th of that month (January), and then suddenly he

exhibited the most extreme dyspnœa. This continued for several days, and the suspicion arose that he might have latent aneurism of the thoracic aorta. The history of the case, however, contradicted that view, and he held the opinion that, however difficult it was to account for the occurrence of œdema of the glottis, that was the cause of the symptoms; and so it turned out to be. The lungs were very voluminous, but there was nothing particular to be noticed. The larynx, which he held in his hand, presented a remarkable appearance. The rima was greatly contracted, the epiglottis was greatly thickened, and the larynx generally was œdematous, and covered over with patches of superficial ulceration. He should have observed that during life albumen was discovered in the urine, which had a specific gravity of 1,011. The kidneys were considerably reduced in size, and, on examination by the microscope, exhibited an excess of fibrous stroma. The heart was very large, and there was an absence of valvular and vascular disease; its hypertrophy was, therefore, to be accounted for by the state of the kidneys. The left ventricle was hypertrophied—not dilated. The right ventricle was full of decolorised fibrin, extending into the pulmonary artery. The case was interesting, as exhibiting a complication of Bright's disease of which he had not previously seen an example—namely, acute œdema of the glottis.—*January 24, 1874.*

*Amyloid Degeneration of the Kidney.*—DR. HAYDEN said a man aged thirty-five, a draper's assistant, was admitted to the Mater Misericordiæ Hospital on the 13th of the present month, much emaciated. He had a slight dry cough, very weak pulse, and weak voice. He had varicose ulcers on the leg, and suffered from constant vomiting and diarrhœa. There was no œdema. The history obtained of the man was that for the last year he had been suffering from cough, and during the same period had been losing flesh. All the measures taken to check the diarrhœa, and enable him to retain food, solid or liquid, failed. He died on the 19th. The kidneys were greatly enlarged, presenting a fine example of amyloid kidney. The liver was also greatly enlarged. The right lung was in a singularly puckered condition. The lower lobe had been the subject of lobular pneumonia at a period long antecedent to death. There was compensatory emphysema. There were likewise patches of recent lobular pneumonia. The heart was normal, except with regard to the presence of decolorised fibrine, presenting an example of thrombosis. The case was of interest in this respect, that with the kidneys in such a state of disorganisation, there was no dropsy whatever; and it was also interesting as showing that this was the form of kidney disease in which there was constant irritability of the stomach. The left supra-renal capsule was enlarged, but on examination with the microscope, it failed to yield any evidence of organic disease.—*January 24, 1874.*

# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

### C O N T E N T S .

---

THIRD SERIES, No. XXX.—JUNE 1, 1874.

---

#### PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. X.—Examples of True Ankylosis of the Hip-joint. By EDWARD H. BENNETT, M.D., M.R.I.A. ; Professor of Surgery in the University of Dublin ; Surgeon to Sir Patrick Dun's Hospital, - -	489
ART. XI.—The Treatment of Hæmorrhoids by the Injection of the Tincture of Perchloride of Iron. By WILLIAM COLLES, Surgeon to Steevens' Hospital, and Lecturer on Surgery in the Hospital College, -	505
ART. XII.—Note on a Lichenoid Eruption occurring on Workers in Flax-spinning Mills. By H. S. PURDON, M.D., L.R.C.P. ; Physician to the Belfast General Hospital and to the Hospital for Skin Diseases, - - - - -	507

#### PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. Lectures on Fever, delivered in the Theatre of the Meath Hospital and County of Dublin Infirmary. By WILLIAM STOKES, M.D., D.C.L., Oxon., F.R.S. ; Regius Professor of Physic in the University of Dublin ; Physician to the Queen in Ireland. Edited by JOHN WILLIAM MOORE, M.D., F.K.Q.C.P. ; Assistant-Physician to the Cork-street Fever Hospital ; ex-Scholar and Diplomate in State Medicine of Trinity College, Dublin, - - - -	510
---	-----

#### PART III.—HALF-YEARLY REPORTS.

Report on Midwifery. By GEORGE H. KIDD, M.D., F.R.C.S.I., L.K. & Q.C.P., lately President of the Dublin Obstetrical Society, and Dublin Pathological Society ; Honorary Fellow of the London	-
--	---



Obstetrical Society; Corresponding Member of the Gynæcological Societies of Berlin and Boston; Obstetric Surgeon to the Coombe Lying-in Hospital. 1. Diseases of the Ovaries; their Diagnosis and Treatment. By T. SPENCER WELLS, F.R.C.S., &c. 2. Ovarian Tumors; their Pathology, Diagnosis, and Treatment, especially by Ovariectomy. By E. RANDOLPH PEASLEE, M.D., LL.D., &c. 3. Histoire des kystes de l'ovaire envisagée surtout au point de vue du Diagnostic et du Traitement, avec un atlas de 24 planches, renfermant 112 figures. Par LOUIS GALLEZ, M.D., &c. 4. Ovariectomy. By J. MARION SIMS, M.D., &c. :—

Ovariectomy, - - - - -	515
Diagnosis of Ovarian Tumours, - - - - -	523
Treatment, - - - - -	527
Tapping, - - - - -	529
Pregnancy Complicated with an Ovarian Tumour, - - - - -	533
Use of Drainage Tube after Ovariectomy, - - - - -	536

#### PART IV.—MEDICAL MISCELLANY.

Transactions of the Medical Society of the College of Physicians:—

The Ophthalmoscopic Appearances of the Optic Nerve in Cases of Cerebral Tumour. By C. E. FITZGERALD, M.D., M.Ch., Dubl.: Ophthalmic Surgeon to the Richmond Hospital; Assistant-Surgeon to the National Eye and Ear Infirmary; Lecturer on Ophthalmic Surgery, Carmichael School of Medicine, - - -	538
On Intestinal Hæmorrhage in Fevers. By HENRY KENNEDY, A.B., M.B.; Vice-President, College of Physicians; Physician to the Whitworth Hospital, Drumcondra, &c., - - -	549

Proceedings of the Dublin Obstetrical Society:—

On Puerperal Convulsions. By THOMAS MORE MADDEN, M.D., M.R.C.S.E.; lately Examiner in Midwifery and the Diseases of Women in the Queen's University in Ireland; Physician to St. Joseph's Hospital for Sick Children; Ex-Assistant Physician, Rotunda Lying-in Hospital; Corresponding Fellow of the Obstetrical Society of Edinburgh, and of the Gynæcological Society of Boston, &c., &c., - - -	560
--	-----

Case of Tetanus following Abortion. By M. A. BOYD, L.R.C.S.I., L.K.Q.C.P.I., M.R.I.A.; Surgeon to St. Michael's Hospital, Kingstown, - - -	583
--	-----

Index, - - - - -	589
Books Received, - - - - -	- Cover.





# THE DUBLIN JOURNAL

OF

## MEDICAL SCIENCE.

---

JUNE 1, 1874.

---

### PART I.

#### ORIGINAL COMMUNICATIONS.

---

ART. X.—*Examples of True Ankylosis of the Hip-Joint.* By  
EDWARD H. BENNETT, M.D., M.R.I.A.; Professor of Surgery  
in the University of Dublin; Surgeon to Sir Patrick Dun's  
Hospital.

THE varieties of true ankylosis of the hip-joint contained in the Museum of Surgical Pathology in the School of Medicine of Trinity College, are sufficiently interesting to repay a careful study. It is much to be regretted that our knowledge of their life-histories, and of their dissection, while recent, is but imperfect. In some instances both are entirely wanting, but this is true of similar specimens in all collections—an almost necessary result of the accidental mode of obtaining specimens of a deformity resulting from disease which has, in most instances, terminated long prior to death.

The first specimen I propose to examine is represented in Plates No. 1. and No. 2. No. 1. represents a front view of the femur and innominate bone; No. 2. the same view, with the interior of the bones and ankylosed joint laid open by a section which passes vertically through the joint, and is slightly curved backwards at the base of the trochanters, so as to enter the medullary cavity of the shaft. A glance at No. 1. shows that this curving of the section was necessary, in consequence of the displacement of the shaft of the femur backwards at its junction with the trochanters.



To prevent error arising in the comparison of the faces of the section,<sup>a</sup> it is necessary to state that a very thin piece of bone has been removed from the anterior side, including in it a part of the canal through the neck. This piece, lenticular in shape, and in circumference about the size of a half-crown piece, was detached in attempting to meet exactly the path of a thin saw carried from below with that of one carried downwards. The deviation of the saws from the plane of the section which detached this wafer-like piece of bone was caused chiefly by the great density of the bone immediately around the canal—a density due, no doubt, to prolonged inflammatory action in the tissue, which rendered the sawing particularly tedious. I exhibited this specimen to the Surgical Society of Ireland on the 10th April, 1874, and gave the following account of its characters:—

I do not hesitate to state that this is an instance of true ankylosis of the hip, consequent to gun-shot fracture of the neck of the femur, and directly induced by the injury which caused the fracture. As far as I have been able to learn, no specimen in any way similar exists, except one contained in the Musée du Val de Grace, a specimen which was presented by Seutin to Baron Larrey, and this, though essentially similar, differs in the gravity of the injury which induced the ankylosis, for while the injury was a bullet wound of the hip-joint, the bone escaped fracture, the bullet only lodging on its surface, and within the joint.<sup>b</sup> That such specimens should be rare, we cannot be surprised at, in the face of such statements of the gravity of gun-shot wounds as the military surgical records furnish. Circular No. 6 of the American War series states:—"Experience having demonstrated the uniform fatality of gun-shot fractures of the head or neck of the femur, when abandoned to the resources of nature, and the excessive mortality of amputations at the hip-joint for gun-shot injury, the highest authorities in military surgery were then unanimous in advising, under suitable conditions, excision of the head of the femur, until, as Baron Hippolyte Larrey expressed it, 'the experiments of the future proved more discouraging than the experience of the past.'"

Guthrie says—"The fractures of the head and neck of the femur are, I believe, always ultimately fatal, although life may be prolonged for some time."

Under these circumstances we cannot, I say, be surprised at the rarity of specimens such as I now present. While I say that I

<sup>a</sup> See Plate 2.

<sup>b</sup> Legouest.—*Traité de Chirurgie d'Armée*.

know but one in any way similar, I do not pretend to deny the possibility of such existing without my knowledge, but I can find none recorded as existing either at home or abroad. That, however, it is possible for patients to recover entirely after gun-shot fracture of the neck and trochanters of the femur, the records of the Hôtel d'Invalides, published by Hutin, prove; for, during one period at all events, there existed four individuals in that hospital who had sustained such injuries, and recovered without operation either of excision or amputation.

I regret that I am not in a position to give any life history of the specimen on the table further than is furnished by itself, but most of the leading facts are recorded on the face of the specimen, as indeed they are on most instances of ankylosis of the hip. It was obtained sometime since in a burial ground of a village not far from Dublin, being accidentally exposed in some repairs of a dilapidated tomb. It became the property of the late Professor Smith, and is now the property of Trinity College. No section of the specimen was made until the late Professor's collection was placed under my charge, and, consequently, no exact knowledge of its nature was obtained. This is the more evident as no mention of it is made in the written notes of the more important specimens which Dr. Smith had made.

The general features of the specimen, its colour, the fractures of the more cancellous portions of the pelvis, and the black deposits in its cancelli—all bear testimony to its long residence in the grave.

The inferior articular surface and extremity of the bone are normal, but the entire shaft is modified by inflammation, and in part by fracture; it is greatly heavier than natural, less cylindrical, and much denser. The anterior surface is wider than that of the normal bone, and its width increases as we trace it up to the trochanteric region. A groove commences near the inner side of the front, at about the junction of the lower with the centre third, and is clearly traceable to the base of the trochanters. Along this groove the surface bears clear traces of the deposit of new bone. On the posterior aspect a corresponding and more copious deposit of bone has taken place along a line which follows the direction of the groove in front, passing up the inner side of the *linea aspera*. This posterior line is widened into a deep fissure as we trace it up to the neck of the bone, where it is suddenly interrupted a little below the large hole that traverses the neck. The

neck and trochanters are greatly deformed, and covered irregularly by bony stalactites that bear evidence of past inflammation. The trochanteric region in front, though masked to some extent by these out-growths, presents three distinct lines of fracture, corresponding to the base of the greater and lesser trochanters, and to the inter-trochanteric line respectively. The position and bearing of the surfaces of the trochanters is altered both with regard to the shaft and neck, and the distance between them is greatly increased, while the lesser is much advanced. The neck is greatly thicker than natural, and is misshapen, while it is pierced by a hole from behind forwards, large enough to admit the top of the little finger behind. A second smaller opening communicates from the front with this passage.

The relations of the axis of the neck of the femur and shaft are altered, while the shaft is displaced backwards and upwards, and rotated outwards. The neck is set on at a right angle, and is directed backwards. The outline of the head and of the acetabulum can be traced over the surface as we pass up to the innominate bone; here the traces of inflammation cease, and no material change is to be noted in the innominate bone. It is that of a male.

The notch of the acetabulum is unaltered and open, and the recess for the Haversian body being empty, the contour of the head of the femur can be seen from without.

The measurement from the brim of the acetabulum to the outer surface of the greater trochanter is greatly increased, as if by hypertrophy of the entire upper part of the femur.

A section made vertically through the ankylosed joint, and inclined so as to traverse the most prominent line of the fracture of the great trochanter, shows that there has been a great comminution of the base of the neck and trochanters, and also a vertical fracture of the shaft of the femur in the direction of the marks of its surface.

The dark colour of the medullary cavity in the photograph almost conceals the fine vertical septum which divides it in the line of this fracture, but any one, on looking closely at the left hand section in Plate No. 2. will see this septum at the upper extremity of the medullary cavity.

The osseous fusion of the bones is complete around the brim of the acetabulum, except at the notch, and for the upper third of the surfaces of the head and acetabulum. The complete fusion of the bones in this part, and the development of the cancelli in relation







to it, while they are completely wasted in relation to other parts, and again, the almost complete obliteration of the traces of the vertical fracture of the shaft, permit us to infer that the subject of these injuries not only recovered their immediate effects, but also lived long after their infliction, and probably made good use of the limb.

Thus far the history, as read from the specimen, is distinct. The owner of these bones lived and died some time ago, was a grown man, and sustained, long before his death, an injury which shattered the neck and trochanters of the femur, and split the shaft vertically for nearly two-thirds of its length; that he probably suffered long from suppurative inflammation of the hip-joint and of the femur, and that he recovered, and made use of a much deformed limb before his death. If these inferences are true the only remaining point to be determined is the cause of the injury.

The holes in the neck, one behind and two in front, communicating with a central cavity, from which the fractures of the shaft and trochanters radiate, suggest, when considered with the exceptional character of these fractures (for they are utterly unlike the fractures, either extra or intra-capsular, of the neck of the thigh-bone caused by ordinary injuries), that they were caused by a bullet passing from behind forwards through the bone, and possibly splintering itself in its passage. At first view the specimen presents on its surface, or in the sections, but little evidence of a positive kind to verify this inference from the characters of the fracture and foramina, and possibly some one sceptical of their truth would attempt to explain away even the evidences of fracture, and set down all the abnormalities of the specimen to disease alone. Convinced of the truth of these conclusions, I made the section to test the external evidences of the fractures by the internal appearances, but chiefly to look for lead.

We know that bullets impacted in bone are not liable to rapid change, and in this specimen I could find no trace of metallic lead in my search; however, I found a mark on the surface of the channel through the neck, which was firmly adherent, white, and hard—too hard to be adipocere, or any animal substance. The condition of the bone, long since freed of the soft parts in the grave, is such, that hardly a trace of adipocere exists within it.

I submitted this white substance to Professor Apjohn, and in his presence detached with the knife sufficient of it to admit of its being analysed qualitatively. Professor Apjohn was as hard to

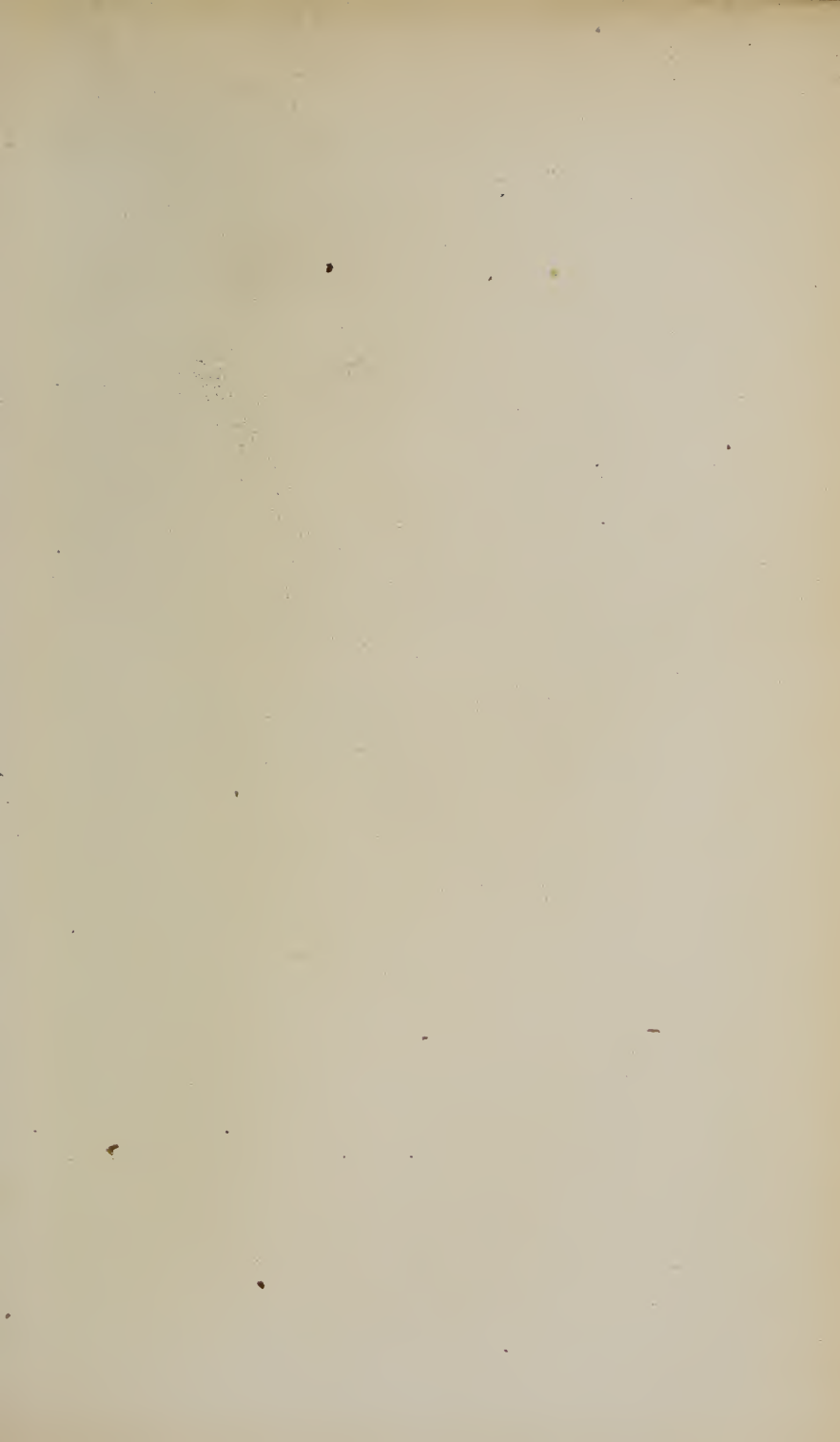
persuade as I was myself of its nature, for he knows better than I do that bullets, or parts of them, are prone to remain unaltered. The tests, however, were convincing, that this material—which still exists in sufficient quantity for re-examination, should any one wish it—is composed of the insoluble lead salts, in the main of carbonate, and probably of sulphate, and of these entirely. We have, therefore, our case fully worked out. We can even tell that this man was shot from behind, for the deposit is directly opposite the posterior opening and beside the place of separation of the channel into two; that is, the bullet split near this point, as it must if entering from behind and splitting.

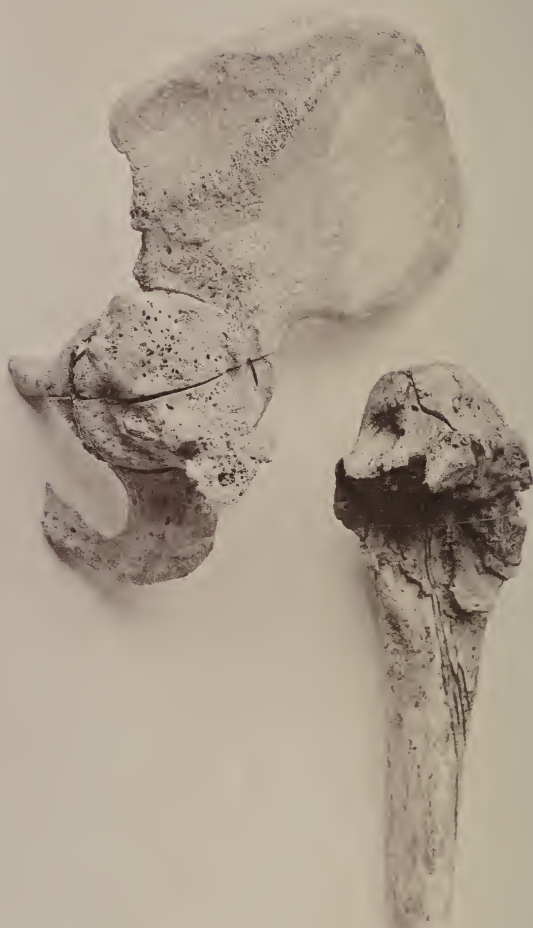
The next specimen in the series is represented in Plates No. 3 and No. 4, and was exhibited at a meeting of the Pathological Society of Dublin during the present session. Of its history or dissection we have no knowledge whatever, except such as the specimen furnishes. It has been for several years in the anatomical collection of Trinity College. Plate No. 3 represents the posterior aspect of the joint. No. 4 shows the innominate bone, with the head and part of the neck of the femur united by true ankylosis, and the upper face of the false joint, which existed in the neck of the femur; in the lower part of the Plate the upper end of the femur is seen with its side of the false joint turned outwards. Sections have been made through each to examine the cancellated tissue of the bones; but as these merely serve to prove the completeness and old date of the ankylosis of the hip, and the absence of any abnormal condition of the shaft of the femur, further than those exhibited by its surface, they have not been figured. The appearance of the innominate bone permits the inference that its owner, a female, had reached her full size before any disease of the hip-joint took place. There is no trace of any intra-pelvic abscess having occurred, nor of any perforation of the bottom of the acetabulum; and there is no excessive thickness of the bone along the line of the brim of the pelvis, a character very constantly present in specimens in which ankylosis has resulted from the so-called strumous form of morbus coxæ. All these points justify the conclusion that the inflammation of the joint which led to the ankylosis was due to some one of the rarer forms of disease which occur after growth is complete—forms which, though rarer in practice than the so-called strumous morbus coxæ, yet much more frequently terminate in true ankylosis. The external surface of the ala ilii, as the Plate shows, suffered from slight inflammation.











The opposite surface presents also, but to a lesser degree, traces of the same action. In the section which has been made through the obliterated hip-joint, passing from the top of the sciatic notch horizontally forwards to the obturator foramen, no break in the pattern of the cancelli indicates the outline of the head of the femur, except at one part, the upper extremity of the depression for the Haversian body of the joint. Here, and for the entire of this space, the original line of the head of the femur is distinctly seen. The plane of the false joint, which intersects the neck of the femur, if the term plane can be applied to so irregular a joint, passes from the border of the acetabulum obliquely downwards and outwards into the lower part of the trochanteric region of the femur. The surfaces of this false joint are similar to those usually found in false joints in long bones, but they are so irregular as to have permitted but little motion of the limb—in fact, but little more than a slight yielding of the limb on the pelvis. A most remarkable feature of the upper part of the femur is the total disappearance of the lesser trochanter, which, if present at all, is to be found only in a small spur of bone near the posterior margin of the shaft. In the absence of any knowledge of the exact attachment of the tendon of the psoas muscle, it appears useless to investigate this point. The external aspect of the great trochanter is only changed by a slight inward inclination of the summit of the process, the result of the altered bearing of the upper extremity of the bone to the pelvis and to the tendons of the glutæi muscles. One point further requires particular notice in this specimen, as I think on it mainly depends the exact determination of the relation of the ankylosis to the false joint. In Plate No. 4 a spur of bone is seen projecting over the centre of the surface of the false joint on the upper part of the femur. The dark shade caused by the sloping plane of the surface, throws this spur out in strong relief. The same spur is equally distinct in Plate No. 3, which shows the femur in the same aspect as No. 4, and with the spur specially defined by the photographer. Its relation to the false joint is seen distinctly in Plate No. 4, namely, that it is part of the femoral side of it. In both Plates it will be seen that a line passes across the base of the spur which separates it apparently from the femur. This line is lost in the path of the section which has been made through the femur. When the bones are fitted to each other, it is evident that this spur was originally part of the lip of the acetabulum and ossified cotyloid ligament. The depression from



which it has been detached is seen in Plate No. 4, below the border of the section of the innominate bone; but the complete verification of its origin can only be obtained by examination of the bones themselves. I have attempted to put the details as briefly and as clearly as possible before my readers; but I fear that without an examination of the bones many will be slow to appreciate fully the importance of this piece of bone in interpreting the signification of the specimen.

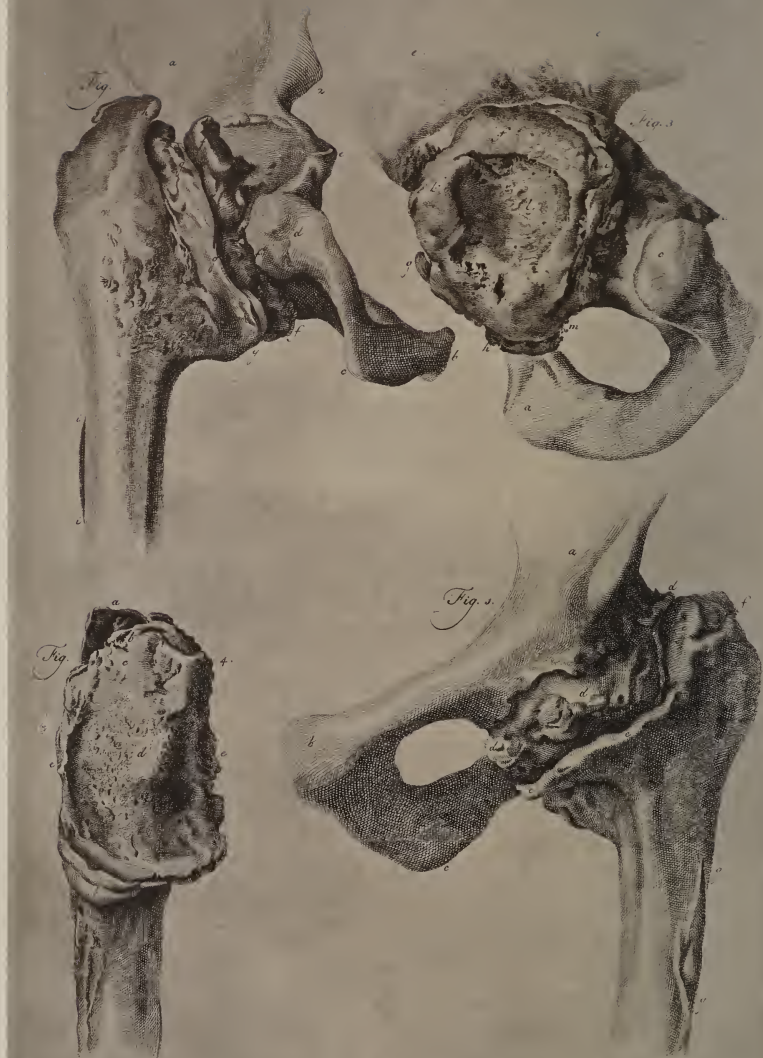
It is evident to any one familiar with the forms of fracture of the neck of the thigh-bone, that the line of the false joint in this case is entirely different from the line of fracture in any of the various injuries, either intra or extra-capsular, commonly met with in the neck of the femur. Yet it is equally clear that the false joint is the result of a fracture. What, then, is the explanation of its exceptional character? I hold, that the only explanation admissible is, that a fracture of the neck of the femur has taken place subsequently to the formation of the ankylosis of the hip-joint. In support of this interpretation of the specimen, conclusive evidence exists in the spur of bone to which I have directed attention above. This could not have been detached from the brim of the acetabulum and united to the lower side of the false joint, except by a fracture occurring subsequently to ankylosis of the joint, nor is the cotyloid ligament, of which this spur was a part, ossified, as it has been prior to the detachment of the spur, by any conditions of the joint except ankylosis, or chronic rheumatic arthritis.

Again, the path of the fracture, although unlike that of ordinary intra-capsular fractures, is strictly within the limits of the attachment of the capsule of the joint, and, therefore, necessitates the conclusion that in this instance true ankylosis, if we regard it as resulting from the fracture, has taken place between the head of the bone and the acetabulum, a condition as yet unrecorded, and apparently the most unlikely that we can imagine, for wasting is the rule in this injury—not the increased action necessary for ossification. In using the words “as yet unrecorded,” I express my dissent to Sandifort’s interpretation of the case figured in his *Museum Anatomicum*, which so closely resembles the specimen which I have been considering that I have reproduced his plate in No. 5, in order that it may be compared with Nos. 3 and 4.

Sandifort appears to have had no knowledge of the life-history of the specimen which he represents, for he says nothing of it in his description, which is as follows:—



LXXIX.



"Os coxæ sinistrum cum suo femore, post fracturam colli, sic degeneratum, ut caput intra acetabulum remanserit, cum eo concreverit, in tumorem inæqualem insignem sese extenderit, et huic pars superior femoris sese accommodaverit."

The words "sic degeneratum," used, I think, in the same sense as I have used the word "wasting," seem to be very inaptly used in the description of a body which has grown into a tumour, and would suggest that Sandifort, who was familiar with the non-union of intra-capsular fractures, considered even the ankylosis a process of wasting. From the resemblance of these two cases, I am inclined to consider them as due to the same cause—namely, fracture subsequent to ankylosis. I think I have assigned sufficient reason for this conclusion in my own case at all events. It is most remarkable that in Sandifort's specimen, the upper part of the femur resembles that represented in No. 4, not only in its general features, but also in the absence of any trace of the lesser trochanter.

The foregoing specimens I have described at some length, in consequence of their rarity. The first not only is very rare, but is also of great surgical importance; the second, though rare at present, may by-and-by be more familiar, for it will be seen that if I have rightly interpreted the specimen, accident has done what has, within the last two years, been done with such good success by Mr. Wm. Adams, and others, in the attempt to relieve the deformity resulting from true ankylosis of the hip, with malposition of the limb—namely, "subcutaneous division of the neck of the thigh-bone."

The remaining specimens of ankylosis, in the collection under my care, are seven in number. Two are undoubted specimens of the so-called strumous morbus coxæ, in which the disease commenced in childhood, and was attended by the formation of abscesses both within and outside the pelvis. Plate No. 6 represents one of these specimens, of which we possess the history sufficiently exactly. I exhibited this specimen to the Pathological Society of Dublin, on February 21st, 1874. The individual from whom it was taken was a boy of fifteen years of age. He had suffered from hip disease for seven years, but a year before his death all the numerous abscesses round the joint had ceased to discharge. He applied for relief of cough and pain in the chest at Sir Patrick Dun's Hospital six months before his death. He was in advanced phthisis, and said that his hip caused but little, if any, pain. The joint was flexed and the thigh adducted. He



was subsequently admitted into one of the Union hospitals, where he died. His body was dissected in the School of Medicine, Trinity College, where I obtained the ankylosed joint. The only remains of active disease in the hip was a small abscess, which occupied a deep depression on the internal aspect of the innominate bone, which was formed by perforation of the acetabulum. This abscess contained an irregular bony sequestrum, which was too large to escape through the opening seen in the front of the specimen.

The neck of the thigh bone in this specimen, as Plate No. 6 shows, was not materially shortened, but it is greatly thicker than natural, in consequence of extensive additions to it by ossification of the capsule, chiefly of the part known as ilio-femoral ligament, which appears to form the lower side of the canal through the joint. The intra-pelvic abscess, formerly much larger than when the specimen was dissected, opened through this canal and also through the anterior and upper part of the great sciatic notch. Its path over the dorsum ilii is seen marked by two deep furrows leading forward, as seen in the Plate, through the attachment of the glutæus minimus muscle. Another and separate abscess groove passes upwards on the dorsum ilii. The disproportion between the size of the ala ilii and the thickness of the bone along the brim of the pelvis is well seen, both in the specimen and in the Plate—a character which I have referred to before, as enabling us to recognise specimens from individuals in whom the disease has commenced in childhood.

The second specimen of this class is in many points similar. The age was about the same, judging by the condition of the epiphyses of the great trochanter and of the innominate bone. We have no history whatever of the patient from whom it was taken. There are in it two perforations from the acetabulum which open into the pelvis, one similar to the last specimen, opening directly into the true pelvis; the second upwards into the iliac fossa, around which latter are traces of a large iliac abscess; both these intra-pelvic abscesses communicated through the upper and outer part of the joint with extra-pelvic collections of pus. Several small sequestra lie imprisoned in the paths of these abscesses to the external surface of the joint. The neck of the femur is greatly shortened, and has sunk into the tissue of the ilium, above the normal position of the acetabulum, to such a degree as to bring the shaft of the femur in contact with the





innominate bone. True ankylosis is, in this specimen, confined to the anterior part of the hip-joint; elsewhere the bones are interlocked, but not united, being apparently kept from fusion by the lingering disease resulting from the several small sequestra confined between them. The limb must have been flexed and adducted to the utmost, for the shaft of the femur is almost parallel to the horizontal ramus of the pubis. Here the subcutaneous section of the neck of the femur would have been impossible—in the former case it would have been almost impossible. Along with these instances of ankylosis resulting from disease occurring in early life, I may briefly examine another specimen of the series, chiefly with a view to the inquiry as to the proportion of cases in which the performance of Mr. Wm. Adams' operation is possible. In this specimen greater facilities for its performance exist than in any other of the whole series; the neck of the femur is not shortened or increased in thickness, nor is the head of the femur sunk into or through the acetabulum; but little displacement of the head of the bone in the acetabulum has taken place; the limb was shortened, judging by the right angle formed by the neck of the femur with the shaft, and adducted. The distinction between the femur and innominate bone can, with difficulty, be traced in the section, so complete is the fusing of the cancelli. No sign of any perforation of the innominate bone by abscess exists, and hardly any traces of caries externally. The only reason for classing this specimen with the two immediately preceding it in this paper is, that the small size of the femur and innominate bone, and the imperfect development of the trochanters, indicate an arrest of growth that suggests that the disease occurred very early in life. The small amount of destruction of bone and the absence of any scars, so common on specimens where abscesses have formed and been cured, suggest that this was an instance of morbus coxæ, terminating in ankylosis, without the formation of abscess, as it does even in youth occasionally. But one half of this specimen exists in this collection; the remainder is one of three specimens of true ankylosis of the hip in the Richmond Hospital Museum, but we learn nothing of the history from the Catalogue of that Museum further than "that it was found in a male subject, aged thirty, brought into the anatomical room of the Richmond School of Medicine." The next two specimens of the collection are so similar that they may be described together. Cheselden's Plate XLVII. of his *Osteographia*, which Mr. Wm. Adams has



re-published, most exactly represents these specimens, except in the displacement of the head of the femur. No two specimens of any of the varieties of ankylosis are exactly alike in this point, but the other essential characters run, as it were, typically through the different species, the varieties being defined by the femoral displacements. Cheselden represents the fully-formed bones of a male united at the hip without any excess of bony growth either in the capsule of the joint or elsewhere, the head of the femur being slightly sunk into the tissue of the innominate bone towards the sciatic notch, this position of the head being associated with slight abduction of the limb. In the first of the specimens I am examining, the limb was in fair position, and the head of the femur projected forwards, so as to be in direct contact with the anterior inferior spine of the ilium. In the second the position of the limb appears (for I did not see it in the recent state as I did the previous specimen) to have been nearly as well placed. The femoral head is, however, further displaced forwards, and lies not in contact merely with the anterior inferior spine, but in front of it, being united to it, and, as it were, concealing it. In both specimens the innominate bones and femora had evidently reached their full size before the disease commenced, as in Cheselden's figure. No trace of abscess or exfoliation exists in them, and the relative size of the bones and the thickness of the pelvic brim is not disproportionate. Cheselden's figure, of which no history exists, represents what might be mistaken for a separation of the epiphysis of the great trochanter, but the line of separation is not that of the epiphysary junction; taken with the defect of the ala ilii, it appears to be a grave yard fracture.

In the specimen before me, which exhibits the greatest displacement forwards, the section of the bones shows that although the bones are of full size and elsewhere complete, the upper epiphysis of the femur had been slowly shifted from its place on the end of the diaphysis, and has been united with it and the acetabulum, with little alteration of its relations to the latter. In other words, the apparent displacement of the head of the femur forwards in this case is really a displacement of the diaphysis only. This appearance is wanting almost entirely in the similar section of the other specimen, a proof that the age of its owner was probably greater than that of the former. I dissected the first of the specimens above described myself, and found no trace of any abscess having existed externally, or within the pelvis. Of the second specimen we have

no history whatever. In both, had the operation of the section of the neck been desirable, it could have been performed with great facility, its only difficulty being the full adult size of the bone.

Mr. Wm. Adams, in his examination of the collection of specimens of bony ankylosis in the Museum of St. Thomas's Hospital, says:—"The eight specimens in this old and valuable collection may be taken as fairly representing the condition exhibited in bony ankylosis of the hip-joint in the different classes of cases in which it occurs." Looking through the series of woodcuts which represents these specimens, it appears to me that Mr. Wm. Adams has overlooked a variety of ankylosis of which I have had the good fortune to dissect a most complete specimen. A second of the same variety exists in the collection under my charge. An illustration in the work above referred to, Cheselden's *Osteographia*, Table 55, Fig. 3, although described as a fracture, is, I think, an instance of this variety of ankylosis resulting from disease. The Museum of the Royal College of Surgeons in Ireland contains two specimens presenting the lesser degrees of development of the same deformity, which is mentioned by several writers on *morbus coxæ*—namely, penetration of the acetabulum by the head of the femur, without material loss of tissue of either head or neck of the bone. Mr. Wm. Adams divides cases of ankylosis of the hip into two classes with a view to the selection of cases suitable to his operation; but this group of cases is not in strict pathological detail referable to either of his classes. He writes as follows:—"In confirmation of the opinion which I have above expressed—that in many cases of bony ankylosis of the hip-joint, the head and neck of the thigh bone remain of their full natural proportions, whilst in other cases they are more or less destroyed, but only in some instances to such an extent as to prevent the operation of the subcutaneous division of the neck of the thigh-bone being performed—I need only refer to the specimens in the various museums of London, where many typical examples of both these classes of cases are to be found."

The first of the two specimens in our collection is the posterior half of a section, the anterior half of which is contained in the Richmond Hospital Museum. I find the following note of it in the Catalogue of the Museum, with the name of Dr. Hutton attached to it as its author:—

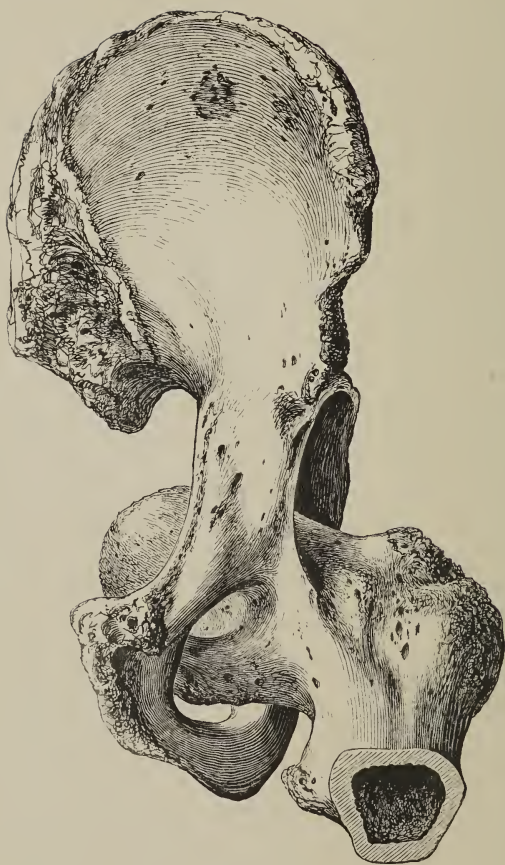
"Thomas Priory, aged fifty, vertical action of the os innominatum

and femur, showing complete bony ankylosis. The individual from whom the preparation was procured had had morbus coxæ in his youth. He died of cancerous disease, affecting the upper curvature and pyloric extremity of the stomach." The shaft of the femur and trochanters are of full size and normal, and the section shows the compact tissue of full thickness, and the cancelli of the neck and head perfect to its very summit. The head and neck have, however, sunk backwards and inwards into the pelvis so far that but one-fourth of an inch of space exists between the internal face of the trochanter major and the border of the acetabulum; only half an inch separates the lesser trochanter from the border of the acetabulum. This displacement of the head of the femur has been effected at the expense of the ischiatic part of the acetabulum. The section shows that the inner table of the pelvic bone has been bent inwards before the head of the femur, and a thin layer of it can be seen in the section covering the entire head. A beautiful example of this in-bending of the inner plate of the bone exists in the Museum of the Royal College of Surgeons of Ireland. In strict detail, this cannot be called a perforation of the pelvis by the head of the femur, for no opening has taken place through the inner table of the pelvic bone. In the specimen before me the spine of the ischium appears as a small projection, as it were, on the top of the head of the femur. In all probability an abscess formed in the buttock in this case, for although no history of it has been recorded in the note of the case, an enfoliation of part of the tuber ischii appears to have taken place. The size and character of the bones show that, although the disease is said to have existed in youth, the growth was fully accomplished before it commenced. The section of the neck could not have been performed in this case, for the saw could not have been worked in the narrow space between the bones. The last specimen of the series is represented in the accompanying wood-cut (Plate No. 7), which was published, with an account of the dissection, in the Transactions of the Pathological Society of Dublin, before which I exhibited the specimen on March 2nd, 1872. The following is the description:—"The body from which it was taken had been that of a man fully sixty years of age. No mark of the opening of any abscess, nor any scars caused by treatment, could be found around the hip and thigh. The thigh was fixed in tolerably good position, slightly flexed and adducted, and the appearances of the foot and limb indicated that the man had walked with facility during life.





No. 7.



The innominate bone and the femur, having been removed from the body, were found to be firmly united by bone. The head of the femur had penetrated the lower portion of the acetabulum and the adjoining part of the obturator foramen, and formed a tumour, which projected an inch into the cavity of the pelvis. The upper and posterior half of the circumference of this tumour was continuous with the tissue of the innominate bone, from the point at which the obturator vessel grooves the pubis in its escape under its ramus, to a point corresponding to the anterior extremity of the tuber ischii. In front and below, the obturator foramen remained unclosed by bone, except for the space of half an inch immediately below the point of escape of the obturator vessels, where a bridge of bone connected the pubis to the circumference of the head of the femur. The internal surface of the true pelvis was normal, except the part occupied by the bony tumour formed by the intruded head of the femur. Externally the femur was united to the ischium and pubis by bone which had been developed in the capsular ligament. In front a strong bar of bone passed down to the inter-trochanteric line, which had the pattern and attachments of the anterior part of the capsule. Behind, the neck of the femur was covered by new bone, marked by circular fibres, also corresponding with those of the capsule. The iliac division of the acetabulum was unoccupied by either the head of the femur or by new bone. In the recent state it was covered in by the capsular ligament, drawn tightly across its opening, and its surface was rough and porous, and entirely without cartilage. The external surface of the ala of the ilium was rough and irregular above and in front, the inner surface being slightly irregular also. The femur showed no traces of disease below the line of the capsule; it was of full adult size, and its tissue showed no sign of wasting. A section, carried nearly vertically through the specimen, showed the cancelli of the neck of the femur of full size and strength, and unaltered by atrophy, except so much of the cone as formed the tumour in the pelvis. This part, which had ceased to support weight, had undergone the usual atrophy of tissue, although it still, in great measure, preserved its normal internal structure. In the section the limits of the neck of the bone and the ossified capsule were clearly defined, and the interval between them was occupied by a perfect cancellated arrangement of new bone. The examination of the specimen led clearly to the conclusion that the original disease had been caries of the acetabulum; that the head of the femur passed into the pelvis,

through the lower part of the articular cavity, without further damage to its own tissue than, possibly, the erosion of the cartilage on its surface. The size and fully developed structure of the femur, as seen in the section, showed that the displacement, and, in all probability, the disease, had occurred in adult life after the growth of the bone had been completed."

In this case no in-bending of the inner compact layer of bone has taken place, and I find that the second specimen of this variety in the Royal College of Surgeons' Museum exhibits the same absorption of the bone, but to a much more limited degree. In these four cases, as in the figure alluded to, contained in Cheselden's work, which I think is wrongly described as a fracture, no traces of comminution of the pelvic bones exist, nor of any fracture whatever beyond the limits of the intruded head of the femur. These fractures, we know, have been observed in the rare injury of dislocation of the femur inwards through the acetabulum from violence, and I can hardly imagine this injury to take place without other extensive fractures, which could not fail to leave their traces on the bones. Specimens that show an in-bending of the inner table cannot, by any force of imagination, be attributed to fracture. I make these remarks, for I know the specimen represented in the woodcut has been by some considered to be the result of fracture. In this case the subcutaneous section of the neck of the femur would have been difficult, though not absolutely impossible. In addition to the specimens I have described above, there are in Dublin, as far as I can discover, eight instances of true ankylosis of the hip—one in the Richmond Hospital Museum, and seven in the Museum of the Royal College of Surgeons—all alike destitute of any history, except the following scant note of one case. The Catalogue gives the history of the Richmond Hospital specimen in these words:—"The patient died of phthisis; he was a lunatic, and had laboured under morbus coxæ in early life." The specimen is but a half of a vertical section, of which so much of the innominate bone has been sawed away that it is difficult to say much accurately, except that the disease probably commenced after the bones were fully grown, and that, if desirable, section of the neck could have been performed. The collection of the College of Surgeons contains four specimens in which the disease occurred in childhood, and three at adult age; in one only of these does the operation of section of the neck of the femur seem an impossibility; it would have been very difficult in one and possible in five.

Taking the entire number of seventeen specimens, the disease occurred in seven in early life before the growth of the bones was complete, and in ten subsequent to the attainment of full size of the bones at least. In one the neck of the femur has been divided; in three its division is impossible; it is difficult in three, and perfectly possible in ten cases.

Mr. Wm. Adams' estimate of the number of cases suitable for his operation, based on an examination of the total number of specimens in London, is, that the operation could be performed in 61·7 per cent. of all hips fixed by bony ankylosis. Our examination of a smaller number, which, however, includes a greater variety of specimens, gives the percentage of possibility 82·3 per cent.; or, if we exclude cases which present exceptional difficulties, 64·7 per cent.—a result which, considering the small number of cases, is singularly close to that of Mr. Wm. Adams.

---

ART. XI.—*The Treatment of Hæmorrhoids by the Injection of the Tincture of Perchloride of Iron.* By WILLIAM COLLES, Surgeon to Steevens' Hospital, and Lecturer on Surgery in the Hospital College.

It is remarkable that, in a disease of such constant occurrence as hæmorrhoids, surgeons should still differ as to the nature and origin of the tumours—some considering them as dilated veins, and others regarding them as mere growths of a vascular tissue. And this difference of opinion has much influence on the treatment of the disease.

From my observation on examining the part when removed, and also considering the nature of the bleeding, which is generally bright arterial blood, and that at times the orifice of a minute artery can be seen pulsating, I have been led to consider them as a vascular growth resembling nævus in children, or erectile tissue in adults.

For the treatment of this disease the surgeon has many plans of treatment—either by medicines used locally or constitutionally, or by various operations. Still, we observe, the advocates of each plan will tell us that they have met cases which resisted the treatment that others advocate as infallible, and that theirs is the best. I beg to add another proceeding in these cases, not to supersede all others, but still, I think, presenting many advantages,



and more likely to be generally successful, and also less liable to dangerous consequences than some others. Some years ago I saw the late Mr. Cusack treat the disease by the introduction into the centre of each tumour of a needle coated with nitrate of silver. I have also often tried this cure; but it causes considerable pain for some days after the operation.

I then thought the same effect would be obtained, with little or no pain, by acting on the internal structure, and treating them, as we do nævus, by the hypodermic injection of some preparation of iron or other ingredient, in order to excite a certain amount of inflammation, and secure the coagulation of the blood in the minute vessels composing the growth, and then subsequent absorption; and such a procedure I adopted in the following case:—

(Reported by F. W. Warren, L.R.C.S.I., L.K.Q.C.P.I., Resident-Surgeon to Dr. Steevens' Hospital.)

Denis R., aged forty-one years, of delicate and anæmic appearance, was admitted to Dr. Steevens' Hospital, March, 1874, suffering from bleeding internal piles.

*Previous History.*—He stated that for the past twelve years he had suffered from irritation about the anus, with a feeling of heat and fulness in the parts, these symptoms undergoing exacerbations at intervals, when an attack of bleeding would give him temporary relief. This state of things continued until a few months ago, when the attacks became more frequent, the hæmorrhoidal tumours protruding during each act of defæcation, accompanied by hæmorrhage more or less severe. He also stated that the bowels were habitually constipated, requiring purgative medicine to keep them regular.

*Symptoms on admission.*—The patient presented a very blanched and emaciated appearance. He complained of a sensation of itching and smarting about the anus, increased to pain during defæcation with considerable hæmorrhage. He stated that he was daily becoming weaker, and presented all the appearance of one suffering from repeated attacks of hæmorrhage. Appetite very poor. On examination (the patient being desired to force down), three tumours (each being about the size of a walnut) protruded, and became visible outside the anus. These tumours presented the usual smooth red appearance. These projections disappeared spontaneously, or with gentle pressure, after defæcation. Ordered

confect. sulphuris, a teaspoonful three or four times daily, so as to combat the habitual constipation, and infusion of rhatany to be applied locally.

March 27th.—Feels somewhat better; the irritation in the anal region being diminished. Bowels regular. As the piles still protrude during defæcation and bleed, an enema of warm water was to-day administered, this causing the hæmorrhoids to protrude; fuming nitric acid was then freely applied. This procedure caused excruciating pain which continued for two hours.

March 31st.—Much in the same condition. States that he does not feel anything better. Had one motion from the bowels since the application of the nitric acid, accompanied by great pain and some hæmorrhage. Bowels confined. Ordered haust. oleosus.

April 11th.—Has not experienced any relief to his symptoms, the mucous projections still protruding during defæcation, accompanied by hæmorrhage and pain. To-day, the piles being forced down, the ordinary tincture of the perchloride of iron was injected, by means of a hypodermic syringe, into each hæmorrhoidal tumour (the quantity injected into each being about 20 minims). The patient was then removed to bed. The injection caused much less pain than the nitric acid.

April 18th.—Expresses himself much better. Has had no bleeding since, and much less pain during defæcation. Ordered suppositoria acidi tannici—one to be introduced into the rectum each night.

April 28th.—Greatly improved. The hæmorrhage, pain, and irritation have completely subsided. Presents a much better appearance. Appetite improving daily. Bowels regular.

May 9th.—The rectum was to-day explored by means of a speculum, and no trace of the piles could be discovered, except three nodules of cuticle, each the size of a shrivelled currant.

---

ART. XII.—*Note on a Lichenoid Eruption occurring on Workers in Flax-spinning Mills.* By H. S. PURDON, M.D., L.R.C.P.; Physician to the Belfast General Hospital and to the Hospital for Skin Diseases.

• THE following brief paper refers to a peculiar skin eruption occurring on those employed in the “spinning-room” of flax-spinning mills. It is principally observed on the arms and fore-arms of the

“doffers.” The affection is trivial, and was first noticed by me in the late *Medical Mirror* for August, 1867; also in the *Journal of Cutaneous Medicine*, where I also described another disease met with in those employed in the same department—viz., onychia, a disease that has attracted a good deal of attention, through the writings of Dr. W. MacCormac, and lately of my former pupil and present colleague at the Skin Hospital, Dr. Johnston Scott.

The affected parts, as already remarked, are chiefly the fore-arms, although the disease occurs on the neck and face. The skin is dry and harsh to the touch, and covered with a papulo-pustular eruption in various stages, intermixed with numberless little black specks, showing the obstructed orifices of gland ducts. When the papules reach the stage of maturity, suppuration at their apices takes place, thus resembling to some extent acne. The eruption has a peculiar “shotty” feel, very like what is felt in small-pox. After suppuration and the discharge of the contents of the pustule the disease disappears for some time, or is exhibited only by the black speck already referred to. The “doffers” and those employed in the spinning-room are lightly clad. The former also oil and cleanse the machinery, and to the oil (either train or sperm) I at one time was inclined to attribute the eruption; but a short time since a very intelligent patient at the Skin Hospital told me that the eruption was more prevalent on the arms when the workers were spinning certain kinds of flax; whilst Dr. Newett, who has had frequent opportunities of seeing this complaint, informs me that a “similar eruption prevails on the people who lift the flax from the ‘holes’ in which it is steeped.” The disease, as already mentioned, is a combination of lichen and acne, so to speak, and I believe it is kept up in a great measure by the relaxed and sluggish action of the sweat and sebaceous glands, allowing sebum to accumulate, as shown by the black spots, which act as “foreign” bodies, giving rise to irritation. I have taken notes of several cases, but do not wish to extend this brief notice by recording them, preferring to give the opinion of two medical friends who have favoured me with their views on this eruption. Dr. Newett, medical officer to Ligoneil Dispensary, Belfast, writes:—“I repeatedly see cases of the lichen-like eruption, chiefly on the arms, if not altogether so, to which you refer;” and Dr. Spedding says that he has “frequently observed the eruption you speak of. During the small-pox epidemic it often confused me when there were symptoms of pyrexia present. The eruption usually remains papular, is frequently in the young

upon the face, as well as on the arms, and might be mistaken for acne. I have observed that it is only those employed in the 'hot rooms' that have it. In fact, from its presence I can always diagnose the patient to be a 'spinner.' I have never tried any treatment for it, believing such to be useless while they are engaged in a warm, moist atmosphere." Drs. M'Crea and John Moore also gave me information regarding this eruption. The treatment of most use is removal from the spinning-room for some time, and either a tincture of tar and black soap, or a lotion containing benzole.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

---

*Lectures on Fever, delivered in the Theatre of the Meath Hospital and County of Dublin Infirmary.* By WILLIAM STOKES, M.D., D.C.L., Oxon, F.R.S.; Regius Professor of Physic in the University of Dublin; Physician to the Queen in Ireland. Edited by JOHN WILLIAM MOORE, M.D., F.K.Q.C.P.; Assistant-Physician to the Cork-street Fever Hospital; ex-Scholar and Diplomat in State Medicine of Trinity College, Dublin. London: Longmans, Green, and Co. 1874. 8vo. Pp. 484.

THE volume before us consists of a selection from the Lectures on Fever which have been delivered in the Meath Hospital by Dr. Stokes during the past twenty years. No man can possibly form just philosophical and comprehensive opinions on epidemic diseases unless his experience of them has been not only large as to the number of cases which have come under his observation, but varied and continued over a lengthened period; and the great value of Dr. Stokes' Lectures lies in the fact, that they embody the views which have been formed by a physician acute in observation and unsurpassed in reflective power, after looking on during half a century at the various visitations of epidemic disease which have passed over this country.

As Dr. Stokes states in his Preface, "these Lectures do not pretend to give even a sketch of all that is known or believed to be known respecting fever;" they rather discuss questions connected with fever, on which, in the author's judgment, erroneous views prevail, or which are specially important in their bearing on practice. The younger members of the profession who become acquainted with Dr. Stokes' opinions, for the first time, from the work before us, will, we fear, not estimate sufficiently the services he has rendered to medical science. It may appear to men who have been educated during the last few years that it is needless to insist on the fact that all diseases are not inflammations to be subdued by anti-phlogistics, or on the fact that head-ache and delirium do not indicate arachnitis, or that the judicious administration of nourishment

and stimulants constitutes one of the most important parts of the treatment of fevers; but it was not so some years ago, and the prevalence of just and enlightened views on these and similar questions, which now happily exists in this country, is due in no small degree to the great clinical teacher whose Lectures are reproduced in this volume. Making fever—from contact with which students naturally shrink—a special subject of investigation, and singularly successful in inspiring them with his own enthusiasm, Dr. Stokes has so assiduously combated the erroneous doctrines which formerly prevailed, that they now linger in the minds of very few practitioners, and his name must ever remain associated with several of the most important additions to our knowledge of essential disease.

The change of type in disease is a subject on which Dr. Stokes, from his long experience, is specially competent to speak, and it is one not only of great interest but of great practical importance. Pictures of disease drawn by the physicians who wrote thirty or forty years ago, have been handed down to us, and the phenomena which they described are still impressed on students as those which they are to expect; while in city practice, at least, there can be little doubt the acute symptoms and highly-marked physical signs which have become associated in our minds with many acute inflammations, seldom present themselves, and, in consequence, practitioners whose conception of them has been acquired from books, often hesitate to recognize them at the bedside:—

“I have not bled,” says Dr. Stokes, “in rheumatic fever for the last thirty years, for the whole character of the disease has changed. We have not had for many years the bounding pulse, the exaggerated heat and sweating, or the same liability to acute inflammations of the internal parts. The action of the heart is often feeble, and the tonic and supporting plan seems called for from an early period. Another point worthy of remark is, that cardiac and aortic murmurs of the anæmic kind have for many years been much more frequently observed, both during the attack and in the convalescence, and these demand the use of iron for their removal.

“In judging of this question the evidence of those who have been intimate with acute diseases in this country during the period of 1820 to 1830 or 1835 must be attended to on this point. As already stated, I have received a vast number of communications from experienced and practical men, who had no theory to support, all telling the same tale, all testifying to the fact that a change in the vital character of acute disease

was observed. This was particularly seen in rheumatic fever, which gradually lost its quality of high reaction, as shown by the great heat of the skin, the bounding and resisting pulse, the vigour of the heart's action; the frequency and severity of metastasis, not only as regards external but internal structures whose functions were violently disturbed. Cases, too, of visceral inflammation, independent of rheumatic complication, and marked by high reaction, pain, and great functional disturbance, were common. This was well seen in the cases of pleuro-pneumonia, in pericarditis, and in peritonitis, in all of which violence of symptoms, high reaction, and great pain and rapidity of morbid processes were the rule. Now, since the asthenic character of acute disease has set in, all this has changed. The violent acute and commonly suppurative pneumonia is rarely met with. We frequently meet cases of pericarditis in which, but for the physical signs, the disease could not be recognized, or its being overlooked would be excusable—you have seen many cases with little or no oppressive distress or throbbing of the heart. And as to acute peritonitis, formerly so well known, there has rarely been seen in any of our wards a case of it that did not result from the perforation of the intestine in enteric fevers."

Dr. Stokes' views on the identity of typhus and typhoid fever are, as most of our readers probably know, at variance with those generally held. He believes that there is no essential difference between them. Many of the facts on which he relies as proofs of this identity are, no doubt, susceptible of a different interpretation, but the question is one on which we could not in this place enter—we allude to it only for the purpose of drawing attention to a point of the utmost practical importance, and on which we feel bound to express no uncertain or hesitating opinion. Insisting, as Dr. Stokes does, on the essential identity of typhus and enteric fever, he fully recognizes the fact that, clinically, the two fevers may be separated:—

"I hope," he says, "that none of you will misunderstand me, and suppose that I do not recognise the differences between the various forms of fever, about which so much has been written in latter times by many excellent observers. That a case of fever answering to the description of the pythogenic fever of Dr. Murchison—the typhoid of Sir William Jenner and Dr. Hudson—runs a different course from a maculated typhus or a relapsing synocha, is to be admitted. All these forms vary, whether in isolated cases or in the multiplied examples which occur in epidemics. As a rule they differ as to their apparent exciting causes, their local complications, their mortality, their attendant phenomena, and their consequent effects on the economy. That follicular disease of the

intestines is commonly met with in typhoid as compared with typhus may be admitted, as well as the rule that the laws of periodicity in the former are not so well marked as in the latter. But what I want you to perceive is that there is between them a great generic resemblance."

For our present purpose the admission of the existence of two groups, whether they be considered varieties of the same disease or essentially distinct fevers, is sufficient; we desire to press on our readers, especially those of them who have charge of fever hospitals, the great difference in the degree and mode of communicability of the two fevers. Dr. Stokes relies on the contagiousness of typhoid as one of the proofs that it does not essentially differ from typhus, and in our review of Dr. Budd's treatise we freely admitted the propagation of the disease by a process which may be described as contagion—namely, by the introduction of the faecal discharges of infected persons into the air breathed, or the drinking water swallowed, by the healthy; but this is not the sense in which we speak of typhus being contagious. The fact that both fevers are communicable from the sick to the healthy may furnish those who look at fevers from Dr. Stokes' philosophical stand-point with an argument for the essential identity of the two maladies. The fact that the modes in which they are communicated from the sick to the healthy differ so entirely furnishes those who are entrusted with the health of families with a guide for action. A healthy man gets typhus fever from another who has the disease if he comes near him, and breathes the emanations from his body, unless these are freely diluted with atmospheric air. The state of health of the recipient, the degree of ventilation of the sick-room, and, probably, the stage of the fever, increase or diminish the danger, but the fact remains that it is by coming near the sick that the disease is caught, and hence the persons who get typhus are physicians, and clergymen, and students, and nurses; and when we are called to a person in typhus, and the duty devolves on us to take measures to prevent the spread of the disease, our duty is to remove the sick person from the house, or to put him in a room away from the other inmates, and insist on free ventilation of the dwelling. But the case is very different with typhoid or enteric fever. A healthy individual may tend one who is stricken with typhoid fever—we do not say without the slightest danger, but with the most trifling risk. The writer of this review slept for three weeks on a bed laid close to a friend in severe enteric fever, and for the greater part of that time received and removed all the evacuations, without



catching the disease, but contracted typhus from a single whiff of undiluted typhus breath. He has treated many cases of enteric fever in the wards of a rather crowded general hospital, without any of the students or the other patients catching the disease. A much smaller number of typhus cases have communicated the disease to students and neighbouring patients in several instances. It is possible, indeed, that if nurses do not wash their hands when soiled with typhoid evacuations before they handle utensils used for carrying food or drink, they may convey the poison to themselves or other patients; but, as a fact, such an event has seldom occurred. "During nine years," says Dr. Murchison, in speaking of the London Fever Hospital, "3,555 cases of enteric fever have been treated along with 5,144 patients not suffering from any specific fever; not one of the latter has contracted enteric fever."<sup>a</sup> Enteric fever is not caught by contact with the sick, but by breathing air, or drinking water, polluted by faecal matters; and hence, when called to a house in which enteric fever has appeared, the steps which, if promptly adopted, would save the unaffected inmates in the case of typhus, would be utterly useless. Removal or isolation of the sick member of the family would not save the others if they continued to breathe the polluted air, or to drink the polluted water. Removal not only of the sick, but also of the healthy, until the sewerage and the water supply have been examined and set right, is the indicated course of action; and, finally, to our minds nothing can be more unjustifiable than the plan which still prevails in many hospitals of placing a man who suffers from enteric fever (or the typhoid variety of fever, as Dr. Stokes would term it) in the wards set apart for infectious diseases. We expose him to the frightful danger of contracting typhus or scarlatina when already weakened by a previous illness, while, practically, he is as harmless to those around him as if, instead of suffering from enlarged follicles in his bowels, he had become an hospital patient on account of a broken bone.

Appended to Dr. Stokes' work are some observations on the use of the thermometer in fever. These are written by Dr. A. W. Foot, and show how he inculcates by example—as, in his Introductory Lecture, he eloquently did by precept—the duty of honest, steady, persevering work, and how worthily he follows in the footsteps of the great physicians whom he has succeeded, and whom he bids fair to rival.

<sup>a</sup> Treatise on Continued Fevers. Second edition, page 463.

## PART III.

### HALF-YEARLY REPORTS.

---

#### REPORT ON MIDWIFERY.<sup>a</sup>

By GEORGE H. KIDD, M.D., F.R.C.S.I., L.K. & Q.C.P.;  
Lately President of the Dublin Obstetrical Society, and Dublin  
Pathological Society; Honorary Fellow of the London Obstet-  
rical Society; Corresponding Member of the Gynæcological  
Societies of Berlin and Boston; Obstetric Surgeon to the  
Coombe Lying-in Hospital.<sup>b</sup>

#### OVARIOTOMY.

WHETHER we hold with the common opinion that the first  
ovariotomy was the case operated on by Ephraim M'Dowell,  
of Kentucky, in Dec., 1809, or that the case described by Robert  
Houston, of Glasgow, in the Philosophical Transactions of 1724,  
as having been operated on by himself in 1701, was really a case  
of ovariotomy, as has been lately asserted by Dr. Mapother, or  
was only a case in which an ovarian cyst was laid open by a free  
incision, as asserted by Peaslee, there can be no doubt the opera-  
tion is one of modern growth—one that has only come to be

<sup>a</sup> 1. Diseases of the Ovaries, their diagnosis and treatment. By T. Spencer Wells,  
F.R.C.S., &c. London: Churchill, 1872. 8vo, pp. 478.

2. Ovarian Tumors: their pathology, diagnosis, and treatment, especially by ovario-  
tomy. By E. Randolph Peaslee, M.D., LL.D., &c. New York: D. Appleton & Co.  
1872. 8vo, pp. 551.

3. Histoire des kystes de l'ovaire envisagée surtout au point de vue du diagnostic et  
du traitement, avec un atlas de 24 planches, renfermant 112 figures. Par Louis Gallez,  
M.D., &c. Mémoire adressé à l'Académie (de Belgique) en réponse à la question  
suivante, qu'elle avait mise au concours pour 1868-1870:—"Faire l'histoire des kystes  
de l'ovaire en insistant sur le diagnostic différentiel et le traitement qui convient à chaque  
espèce de kyste." Le prix, consistant en une médaille d'or de mille francs, a été  
décerné à l'auteur. Bruxelles, Manceaux, 1872. 4to, pp. 1,000.

4. Ovariotomy. By J. Marion Sims, M.D., &c. New York: Appleton. 1873.  
8vo, pp. 85.

<sup>b</sup> The author of this Report, anxious that every contribution to Midwifery should  
be noticed, will be glad to receive publications on the subject. They may be sent to  
the publishers of the Journal through their correspondents.

recognized as a part of orthodox surgery within the last fifteen years—and that it is to the labours of Spencer Wells it is indebted for this recognition.

The opposition he had to face in establishing the operation in this position would have daunted less earnest and courageous men. The coarse and vulgar language of one eminent teacher who denounced all ovariomists in language scarcely fit to be repeated, or the violence of another who threatened his colleague, Mr. Baker Browne, with the terrors of a coroner's inquest and legal proceedings if any patient on whom he performed ovariotomy in the hospital should die, were difficulties of little importance; but the arguments of Dr. West, in the early editions of his work on the Diseases of Women, and those of other calm and rational men, were, from the manner in which they were stated and the character of the authors, not to be faced by any one who had not great resolution and confidence in his own judgment. The result, however, has justified the boldness of the man. Marion Sims, whose work is one of those now before us, says that he himself was "among the great herd that could scarcely find terms strong enough to condemn what were then characterised as acts of butchery and murder." Baker Browne's colleague became himself an enthusiastic ovariomist, and Dr. West, in the later editions of his work, has acknowledged himself converted by the irresistible logic of facts, and instead of opposing the operation, advocates its performance in all suitable cases. The contest is not now for who can denounce ovariotomy in strongest terms, but rather as to who has done the most to advance it.

"Seven cities warr'd for Homer, being dead,  
Who, living, had no rooffe to shrowd his head."

So urgent have been the claims put forward for this honour, that it has been necessary for one who has been himself one of the most successful operators to pronounce judgment on them. In this judgment of Mr. Keith's we fully concur. To Mr. Wells he unhesitatingly gives the credit:—"And when I think," he says, "of the weariness of flesh and spirit with which Spencer Wells' great work has been accomplished—and there has never been anything like it in surgery since surgery began—it seems to me strange that any doubt could have arisen at all."

The whole history of the operation is most interesting and full of instruction. The removal of the healthy ovaries had been effected, sometimes for corrupt and unworthy purposes, from an

early period, and surgeons have cut down on and even excised the healthy organs when lying accidentally in the inguinal canal. The first suggestion, however, to remove the ovaries for the cure of cystic disease was made in the year 1680, by Professor Plater, of Basle, and supported by Schorkopff in 1685, who thought it would be a certain cure "if it did not appear so cruel and so dangerous." From this time the idea was discussed by the leading authorities in Europe, in the Academy and elsewhere, but without practical result. John Hunter thought it justifiable, and said "there was no reason why women should not bear spaying as well as other animals;" and John Bell, of Edinburgh, in his lectures, dwelt with peculiar force and pathos on the hopeless character of ovarian tumours when left alone, and the practicability of removing them by an operation. Among the pupils of the bold and enthusiastic Scotchman there was, in 1794, a young Kentuckian, of the same name as a surgeon once well known among ourselves, Ephraim M'Dowell, who determined to put in practice the teachings of his master. He set himself to study thoroughly the relations of the pelvic viscera in their healthy and diseased conditions, but it was not till 1809 that he met with a suitable case for trying the operation. Fortunately his patient recovered, as did three others in succession, but the fifth died. He performed the operation in all thirteen times. Of the number of deaths there is no exact information, but there were certainly eight recoveries, if not more.

Another American—Nathan Smith, of Connecticut—without knowing what M'Dowell had done, operated successfully in 1821; and Dr. A. Smith, who had assisted M'Dowell in some of his cases, had one of his own, also successful, in May, 1823. After this the operation was performed in many parts of America; and in 1825, Lizars, of Edinburgh, having heard of M'Dowell's cases, operated on two patients, the first of whom recovered, but the second died in fifty-six hours. On two other occasions he opened the abdomen, but only to find the diagnosis had been erroneous. Two years afterwards, Granville, of London, operated in two cases—in one the tumour was not removed, the adhesions being too extensive; in the other the tumour proved to be a uterine fibroid, and was removed, but the patient died on the third day.

The ill success of Lizars and Granville deterred British surgeons from undertaking the operation till 1836, when two provincial surgeons, Jeaffreson and King, operated successfully, adopting the short incision suggested long before by William Hunter. Other



provincial surgeons had single cases, and Mr. West had, in 1839, a second successful one, one unsuccessful, and one incomplete. In the same year Mr. Morgan operated in London, at Guy's Hospital, but could not remove the tumour, the patient dying in twenty-four hours; and in 1840 Mr. Benjamin Philips removed an ovarian tumour in Marylebone Infirmary, but the patient died. In 1842 Dr. Clay, of Manchester, began his series of cases with three successful ones out of four, and in the same year Mr. Warne performed the first successful ovariectomy in London. In the following year he had two more. Mr. Lane, Dr. F. Bird, Mr. Cæsar Hawkins, and other London surgeons, operated at this time, but few other London surgeons followed their example, though successful cases continued to be reported by Dr. Clay and other provincial surgeons. In 1852 Baker Browne operated on his first case, and between that and 1856 operated on nine patients, of whom seven died, a result strangely in contrast with that of his later years. He then ceased to operate for four years, partly, it has been stated recently, because of the opposition of his colleagues in St. Mary's Hospital, and partly from feeling that a general hospital was not a suitable place for ovariectomy. He resumed his operations in the Surgical Home on the 20th of October, 1858. Between 1852, his first case, and 21st May, 1868, the date of his last, he operated on 121 patients, with the result of 35 deaths and 86 recoveries. Of his first 50 cases he had 24 deaths, and only 11 in the last 71.<sup>a</sup> However, it is stated by Mr. Keith (*Brit. Med. Jour.*, 20th Dec., 1873) that he had 10 other deaths from incomplete or partial operations during the same period.

Mr. Wells' first operation was undertaken in December, 1857, and was not completed, owing to the intestines being found anterior to the tumour. The incision was closed up, and the patient recovered, but died four months afterwards from the spontaneous rupture of the tumour into the peritoneum. In February, 1858, he repeated the attempt, and cured his patient. The following year, 1859, was, however, an unfortunate one for ovariectomy in England, for, according to Dr. Peaslee, there were eighteen deaths out of twenty-seven cases. Eleven of these were operated on by Mr. Wells, and five of them died. In 1860 there were twenty-four cases and eleven deaths; and in the following year thirty-six operations and eleven deaths again, eleven of the

<sup>a</sup> Letter from Lennox Browne, *Brit. Med. Jour.*, 13th Dec., 1873.

operations having been by Mr. Wells, with a result of six recoveries and five deaths. In 1862 there were fifty-nine cases, and twenty-four deaths, Mr. Wells having twenty of the cases, and only five deaths; and next year he had fifteen cases, and three deaths, the total operations for the year being thirty-six, with nine deaths. The turning point was now fairly reached, and the labours of Mr. Wells began to be crowned with success. When he began to test ovariectomy by personal experience, he pledged himself to make the results fully known to the profession, and to publish every case, whether successful or not. This promise he has scrupulously fulfilled, and by so doing has perhaps contributed as much to the establishment of the operation as by any other part of his labours. In the book before us he gives the result of 500 cases of completed ovariectomy, in 25 of which both ovaries were removed. Of the 500 patients, 373 have recovered from the operation, showing a mortality of 25·4 per cent., which is smaller than that of many capital operations, which are constantly performed in suitable cases without hesitation.

Mr. Wells has traced the subsequent history of the majority of those who recovered:—35 of them, who were unmarried at the time of the operation, have married since. Of these, 14 have had 1 child; 6, 2; 3, 3; and 3, 4 children, and 2 have had twins. Of 259 who were married at the time, 23 have had 1 or more children since. Of 25 no information could be obtained. 310 of the patients were in good health in 1872. Many of them stated they were well and strong, or better than they had been for many years; some complained of trifling ailments; others had died at various periods of disease, in some connected, in others not in any way connected, with the operation.

“When we consider,” says Mr. Wells, “that a patient from whom an ovary has been removed can scarcely be said to be mutilated, as she is perfectly capable of fulfilling all the duties of a wife and mother, menstruating regularly, and bearing children of both sexes without any unusual suffering, either during pregnancy or labour all doubt as to the legitimacy of ovariectomy must be at an end.”

In Scotland ovariectomy was entirely discountenanced after the experience of Mr. Lizars, and not repeated for twenty years, when Dr. Handyside, of Edinburgh, operated (1845), but unfortunately without success. This case was brought forward at the Medico-Chirurgical Society of Edinburgh in a paper by Dr. Bennett, and the subject was discussed at the next meeting (December 17,

1845), when Sir James Simpson—then Professor Simpson—made his eloquent and conclusive defence of ovariectomy as a justifiable operation. His remarks were published in the *Edinburgh Medical Journal*, and contributed in a great degree to the establishment of the operation as one right and justifiable. In 1857 the subject was again discussed in the same Society, and Simpson's argument in favour of the operation was opposed by Dr. J. Matthews Duncan, who, not content with the discussion, followed it up by a paper entitled, "Is Ovariectomy justifiable?" published in the *Lancet* in February, 1859. To this Simpson replied at some length, but he did not notice two other letters of Dr. Duncan's, published soon afterwards in the same Journal. Dr. Duncan denounced ovariectomy as unjustifiable, and characterised Simpson's arguments in its favour as "flimsy and fallacious," and boasted of the paucity of the ovariectomies in Scotland at that period—"a paucity depending, not on the cowardice of our surgeons, but on the feelings of our hearts, and the conviction of our intellects, that the operation is unjustifiable." The progress of events has furnished a very striking commentary on this controversy, and there are probably but few Scotchmen who would not now refer with pride to the part taken by their countrymen in establishing the operation—to the teachings of the Hunters, of Bell, and of Simpson, and the successes of Keith, as proving it to be not only justifiable, but one the surgeon is bound, in deference to the feelings of his heart and the convictions of his intellect, both to recommend and perform. To Dr. Thomas Keith, a Scotchman, has fallen the greatest amount of success yet attained in Europe—81·61 per cent. His first operation was performed in 1863, and in January, 1872, he had operated on 136 patients, and, in the opinion of Dr. Peaslee, to him must be awarded the highest order of skill, both as an operator and in the after-treatment of ovariectomy cases, since the reports of his cases show that they have by no means been usually of a promising character, but very often quite the reverse. Keith, however, is by no means the only operator in Scotland, for M'Leod, Buchanan, and others, have followed his example.

Dr. Peaslee, whom we have in a great degree been following in the foregoing sketch of the history of the operation, has been strangely misinformed as to what has been done in Ireland. Ovariectomy, he says, had been but twice successfully performed here up to the last quarter of 1863, and is even now but seldom

attempted. "A letter," he says, "of September, 1871, from one of the most distinguished hospital physicians in Dublin, states that Dr. Hayes, of the Mater Misericordiæ Hospital, recently had a successful case of ovariectomy, and proposes soon to operate again. He had, however, himself hesitated to advise the operation, for reasons which, considering them entirely obsolete, I shall not here repeat. Nevertheless, he alludes to the experience of Mr. T. S. Wells, as if familiar with it." Whoever this Dublin physician may be, and however familiar he may be with the experience of Mr. T. Spencer Wells, he certainly is not familiar with what has been going on in Dublin, for there is no hospital in Dublin in which the operation has not been done, and the Dublin surgeons have no hesitation in either recommending it, or undertaking its performance. The disease, however, is probably not so common in Ireland as in countries where great numbers of women are drawn together, and obliged to lead unnatural, unhealthy lives in crowded factories. Whether from this reason or some other, the operation has not been followed by the same amount of success in Ireland as elsewhere. It seems not impossible that when the disease appears at rare intervals, and is not the result of special exciting causes, it occurs only in individuals of weak organization, and but little able to bear up against a serious operation. Be this as it may, the success of the operation in Dublin has been very low. The operation has, we believe, been performed in Dublin in 49 cases. In 35 of these the patients died, and in 14 recovered. It has been performed by 20 different operators, all of them men of repute, and some of them of the very highest standing as operating surgeons, and the measure of their success has been very nearly equal. In 6 of the 49 cases the operations were performed by two of the most successful ovariectomists of the age, and yet their success in these cases was not as great as that of most of the Dublin surgeons. Of these 6 cases one was operated on by Dr. Charles Clay, of Manchester, and the patient died within 24 hours. The remaining 5 were operated on by Mr. Wells, and of these only 1 recovered.

We have not space to follow the history of the operation further. Dr. Peaslee traces it very fully throughout America, the different countries of Europe; and elsewhere; and he has tabulated 739 known cases in the United States of America, occurring during the seven years from 1864 till 1871, and 1,006 in Great Britain, giving the names of the operators and the per-centage of success obtained by each, being in America, on the whole number, 68·63 per cent., and



73·75 in Great Britain. This is an amount of success sufficient to justify the suggestion of a recent biographer of M'Dowell—that the women, whose lives had been saved by this great operation, should raise a monument to the memory of this great surgeon, who first, with pre-established knowledge, well-matured judgment and skill, determinedly undertook the performance of this great operation. Dr. Peaslee sums up his statement of the considerations in favour of the operation with the following somewhat curious calculation:—

“ 9. Finally, ovariectomy excels all other strictly surgical procedures in its life-prolonging results to woman. I exemplify this proposition from the experience of Mr. T. S. Wells. The average age of the third one hundred patients ovariectomized by Mr. Wells was very nearly thirty-nine (38·97) years; and at the age of thirty-nine the expectation of life for a woman in health in England and Wales is, according to Dr. Farr's table of the mean duration of life,<sup>a</sup>  $28\frac{5}{12}$  years. Had no operation been performed, ninety-five (Boinet says ninety-eight) of these women would have died in less than an average of four years, while the remaining five might possibly, but not probably, have averaged eight years each, of life. This would give an aggregate of  $(95 \times 4) + (5 \times 8)$  four hundred and twenty years of life for the whole one hundred persons.

But Mr. Wells saved seventy-seven of his third one hundred cases, and thus added to their lives  $(28\frac{5}{12} \times 77) - (4 \times 77)$  1,880 years; while the loss of life on the part of the twenty-three who did not recover from the operation, if we assume the high estimate of four years for each, was ninety-two years. The result, therefore, of the third one hundred operations by Mr. Wells is the actual bestowal of  $(1,880 - 92)$  1,788 years of life on his one hundred patients, more than they would collectively have experienced had no operation been performed. And if we date from the operation only, with the seventy-seven who recovered, we find that  $(77 \times 28\frac{1}{2})$  2,188 years of average healthy life has been given them, instead of the  $(77 \times 4)$  three hundred and eight years of misery they would have suffered up to their death; and of the four hundred and twenty years of miserable existence the whole one hundred would have passed.

“ Considering that the preceding estimate is lower than is justified by the fact—for the tables of Finlaison would have made the result at least twelve per cent. higher—it is a modest claim for Mr. Wells, that he alone, by his operations as an ovariectomist, now numbering four hundred and fifty, has added more than  $(1,788 \times 4\frac{1}{2})$  8,046 years to the aggregate lives of his patients, and secured to the survivors  $(2,188 \times 4\frac{1}{2})$  9,846 years of average health, in lieu of  $(308 \times 4\frac{1}{2})$  1,386 years of suffering.

<sup>a</sup> Chambers's Encyclopædia, Article, “Mortality, Law of.”

"By a similar calculation, upon the data on pages 248 and 276, it may be shown that, in the United States and Great Britain alone, ovariectomy has, within the last thirty years, directly contributed more than thirty thousand years of active life to woman; all of which would have been lost had ovariectomy never been performed.

"When Mr. T. S. Lee termed ovariectomy 'an operation without its parallel,'<sup>a</sup> he alluded merely to its formidable surgical aspects; for its real value could not, in 1847, be appreciated. But all can now agree with Grenser when he terms it an operation fraught with happiness; with Dr. Keith, who thinks it a splendid operation;<sup>b</sup> and with Koberlé, who terms it 'un des plus beaux titres de gloire de notre époque chirurgicale.'"<sup>c</sup>

#### DIAGNOSIS OF OVARIAN TUMOURS.

The diagnosis of abdominal tumours was so little studied fifty years ago that the ovariectomies attempted at that time often served but to demonstrate and correct errors that had been committed—errors, the occurrence of which retarded not a little the adoption of the operation. In 1823, Lizars laid open the abdomen of a young woman, twenty-nine years of age, who was supposed by several eminent physicians to suffer from ovarian disease; but it was proved that the abdominal enlargement was due only to tympanites and obesity. Two years later, he opened an abdomen to find a large uterine tumour. Similar mistakes have been made by many others, of which Dr. John Clay, in his Statistical Tables, mentions no less than twenty-one examples. But, partly by the aid of these, by careful observations, faithfully recorded, and by much study, the diagnosis of ovarian tumours has now arrived at such a degree of perfection that much that was formerly obscure is now a matter of intuitive perception. There are, however, many matters of detail on which we are still at fault, some of which the most experienced observers admit they even yet know of no means of deciding, except by actually cutting down on the tumour. Mr. Wells gives the following enumeration of the principal states and diseases which may throw doubt on the diagnosis of a case of ovarian tumour, or for which it may be mistaken:—

<sup>a</sup> *Op. cit.*, p. 115.

<sup>b</sup> "I think it a splendid operation. At the same time I feel that I somehow know less about it than I seemed to do some years ago. There is yet much to learn about it."—(Letter to the author, November, 1869.)

<sup>c</sup> "De l'Ovariotomie," p. 34.

“In connection with the peritoneum we have—

Ascites,

Cancer and tubercle and encysted dropsy of the peritoneum,

Tympanites and phantom tumours,

Fibro-plastic tumours of peritoneum, and

Fatty tumours of omentum and mesentery.

“Difficulties in diagnosis caused by uterine enlargements arise from—

Pregnancy,

Retained menses and moles,

Air and fluids in uterus,

Fibroid tumours,

Cancer.

“Another miscellaneous group is this—

Enlargements of other viscera, such as the liver, spleen, and  
lumbar and mesenteric glands,

Hydatid cysts of the liver and peritoneum,

Movable kidney and cysts of the kidney,

Fæcal accumulations,

Distended bladder,

Hæmatocele,

Pelvic abscess,

Extra-uterine pregnancy,

Enchondroma, or encephaloid disease of ilium or vertebræ.”

Mr. Wells describes, with the hand of a master, and with the clearness of one who has himself worked it out, the means of distinguishing between ovarian dropsy and each of the foregoing conditions. He does not hesitate to tell of the cases in which he has failed to make the diagnosis. On the contrary, he records them fully and minutely, and thus lays the foundation for future success. All the senses must be used in obscure cases. Inspection, palpation, percussion, and auscultation, aided by mensuration, must all be used. By the uterine sound much can be learned, as well as by vaginal and rectal digital examination; but, of all these, palpation and percussion afford the most valuable information. By passing the hand over the abdomen, the presence of a tumour is ascertained, also whether it is solid or fluid, or partly the one and partly the other; whether it is movable or immovable, smooth and regular in its outline, or the reverse; but, by percussion, it must mainly be decided whether the swelling is the result of ascites or of ovarian disease, or caused by tympanites, or by a phantom tumour. In ascites, the stomach and intestines, containing air, float on the surface of the fluid, and, therefore, the highest points of the tumour,

the patient lying on her back, give out a clear sound on percussion. If, however, the fluid be contained in a cyst, the stomach and intestines are pushed aside, as the tumour rises in the abdomen, and lie in the epigastric and two lumbar regions. Hence, the highest points of an ovarian tumour emit a dull sound when percussed, and the epigastric and lumbar ones give a clear sound. By applying these general rules in any ordinary case, a few seconds will enable a surgeon to clear up all doubt. But Mr. Wells says there are various conditions that may lead to the necessity for further examination. The abdomen may be so much distended by ascites that the intestines cannot float to the surface, the mesentery being too short to allow of their doing so; or the intestines may be tied down by adhesions, the result of peritonitis, probably of a tubercular or, perhaps, of a malignant character, and itself the cause of the ascites. In either case there will be free fluid in the peritoneum, and yet the highest part of the tumour will give out a dull sound on percussion. In another class of cases an ovarian cyst may contain air as the result of a communication with the intestine, or of decomposition of its contents after tapping, and consequently a clear note will be elicited by percussion. In these cases, Mr. Wells says, physical diagnosis alone will not solve the doubt, and all that can be learned from the history of the case, and general condition of the patient, must be considered in forming a diagnosis. There is another condition that might be classed with those mentioned by Mr. Wells. An over-distended bladder may rise in the abdomen as high as the umbilicus, filled partly with ammoniacal urine and partly with gas, the result of the decomposition of the urine, and present a large fluctuating tumour, dull on percussion in its lower part, and clear at the sides and above, so as to closely resemble an ovarian tumour; but the use of a catheter will at once remove the swelling, and, with it, all doubt as to its nature.

It was for a time hoped that a chemical and microscopical examination of the fluid obtained by tapping might assist the diagnosis. After stating very fully all that may be learned by these methods, Mr. Wells shows that complete reliance cannot be placed in them. Following up the observations of Thudichum on the appearances presented by luteine, the colouring matter of the corpus luteum, Dr. Peaslee hopes for more valuable and reliable information from examinations with the spectroscope, but this part of the subject has not yet been fully explored.

Dr. Peaslee mentions a condition, as occasionally occurring, which



we have ourselves so frequently observed that we always look for it as characteristic of the presence of free fluid in the peritoneum, either with or without an ovarian, or other tumour, and we are surprised Mr. Wells does not allude to it—this is a prolapse of the posterior wall of the vagina, from the pressure of the fluid in Douglas' space. It is the upper portion of the posterior wall of the vagina that is prolapsed, the part covered by the recto-vaginal pouch of the peritoneum. It is easily distinguished from the tumour formed by prolapse of the lower part of the vagina known as a vaginal rectocele, by passing the finger into the rectum and thence into the pouch from which the tumour receives its name. We have scarcely ever seen a woman whose abdomen was much distended by ascites in whom this prolapse had not occurred. It is also found where there is an enlarged ovary or other tumour surrounded by ascitic fluid, but never in cases of simple ovarian or other tumour. The umbilicus, too, is often protruded as a hernia by ascitic fluid, either with or without an accompanying ovarian tumour. Mr. Wells states it is often flattened, as in pregnancy, when there is an ovarian tumour, but never prominent unless there be ascitic fluid surrounding the ovarian tumour; but we have ourselves seen at least two cases where the umbilicus was made prominent by the protrusion of a portion of a subjacent tumour. We have also met with a reducible omental hernia at the umbilicus, in connexion with an ovarian tumour, and by this recognized the existence of an adhesion between the great omentum and the tumour, the presence of which was proved during the operation.

Localized collections of fluid in the peritoneum may cause much difficulty in diagnosis. Mr. Wells gives the details of one case of this kind in which he believed there was a multilocular ovarian tumour, and only found his mistake on opening the peritoneum to remove it. He also quotes two similar cases from the American journals which occurred in the practice of Dr. M'Dowell and Dr. Henry Smith. These collections may be associated with cancer and tubercle of the peritoneum, and the symptoms so produced have, Mr. Wells says, so closely resembled those met with in many cases of ovarian cysts as to deceive men of the greatest experience. "Even among my own cases," he says, "the co-existence of cancer has been so masked by the symptoms of ovarian disease that one has been led on by the hope of giving operative relief." And he narrates two cases in which he himself was thus led on by this delusive hope. Other abdominal tumours may resemble ovarian

cysts in their general characters, and cannot always be distinguished. Of these Mr. Wells records several examples, and gives the details of some in which the resemblance was so close that an operation was attempted for their removal. One was a fibro-cystic tumour of the uterus on which he operated in Dublin, and another a renal cyst. These cases are so carefully analysed and described that they serve as landmarks to save us from similar errors in the future, and to indicate the points whence fresh progress has been made and new knowledge been acquired. There are, however, still some cases, it must be admitted, in which an accurate diagnosis cannot be arrived at without making an exploratory incision and exposing the tumour. The general principles of diagnosis are now, however, so well established that these latter cases are very few, and are every day becoming still more rare.

#### TREATMENT.

As to the medical treatment of ovarian disease, all authorities now agree with Hunter in admitting its inutility, whether or not they are inclined to turn with him to the sister art of surgery for the aid medicine cannot afford. Wells, introducing a chapter in which he discusses the value of some of the much-vaunted remedies for the disease, sums up very accurately all that can be said on the subject in the following remarks, and it would be well the rule he advocates were adopted to prohibit any medical treatment which could possibly injure the general health of the patient or place her in a less favourable condition than she otherwise would be for such surgical treatment as may ultimately be called for:—

“I do not say that medical treatment is of very little use in cases of ovarian tumour, simply because I am a surgeon and can remove the disease. But on looking over the medical literature of the subject, one finds the key-note of this chapter always the same—hopeless impotence. Of course, among the obscure writers or the vain seekers after notoriety, there is the usual jangling discordance of ignorant vaunting of this or that remedy, which gives as little pleasure or as much disgust as the Chinese part of a concert. But the dicta of the great authorities have a melancholy accord, curiously modulated by the circumstances or temperament of the speaker. Hunter, while frankly admitting the inutility of the medical science of his day, turns with prophetic expectancy to the sister art of surgery. We have seen how pathetically such a man as West resigns his patient, and deplores the uselessness of his interference. The cautious, investigating, and practical mind of Watson, compelling

him honestly to confess that he is 'unable to reckon one single instance of success,' leads him at the same time to urge that we are bound to try what medicine can do in such cases, since, as some have done well, 'we scarcely know how or why,' it is still possible that we may, in some way or other, succeed again. On the other hand, the dogmatic Simpson, having observed and demonstrated the hurtful and injurious influence of experimental physic, authoritatively says:—'Be very chary in the administration of drugs, and especially of strong and heroic drugs, in cases of multilocular ovarian dropsy,' and adds, that one of the great and difficult lessons in practice is to learn 'to know when to stay and withhold your hand, and to keep yourself from injuring your patient by overconfidence in medicine, and by the too great use of physic.'

"British experience is echoed from the Continent. Neither climatic influences, nor constitutional peculiarities, seem to assist the physician in his efforts at cure. One might expect something different from the impetuous science of the South, acting upon the warm-blooded victims of ovarian disease, in a climate whose morbid and curative alternations are almost phantasmagoric, or from the persistent therapeutical batteries and sieges of the North, opposed to the passive resistance of phlegmatic habits. But on the scale of medical success the index stands nearly everywhere at zero. If Courty at Montpellier thinks that oxide of gold has put him two cases in advance of Sköldberg at Stockholm, Boinet at Paris looks into the matter and comes to the conclusion that, 'Malgré l'estime bien grande que nous avons pour la science de notre savant confrère M. Courty, on doit attendre que d'autres faits mieux diagnostiqués viennent montrer l'efficacité de l'oxyde d'or dans les kystes de l'ovaire.' Kiwisch is obliged to be content with a simple symptomatic treatment; Engelmann pointedly declares that 'there is as little benefit to be expected from the Kreuznach waters as from any other medical treatment;' the high transcendentalism of Berlin admits of no specifics; the plodding nihilism of Vienna only fertilises the field of ovarian pathology; while at St. Petersburg professional and artistic skill expend themselves in the most elaborate operational illustration. The word ovariectomy is positively magnetic at New York—and, in the land of McDowell, who would think of anything short of the radical cure by operation of native origin? So at last we are reduced medically, as Dr. West says, to the consideration of two classes of cases, the first of which *may* be let alone, and the second which *must* be let alone; a third presenting itself in a form which justifies or absolutely requires interference, and that interference necessarily operative.

"The sum of medical doctrine therefore on the subject amounts to this: palliate where you can; do no mischief where you cannot. The general state of health of the patient is obviously the first consideration; every attention is to be paid to it. All matters of diet, hygiene, tonics

for the body, and consolation for the mind, are to be regulated and administered under the conviction, that whatever tends to support the strength and cheer the spirits of the patient, does as much as can be done in arresting the progress of a disease which, in its essentially parasitic character, flourishes under despondency and preys upon weakness. Though these cases are not utterly hopeless, and do sometimes spontaneously come to a standstill, yet when steady progress can be observed from time to time, it is better at once to put aside the old presumptuous talk about deobstruents, discutients, evacuants, and such like delusively promissory inanities, to disabuse the mind of vain expectations, to seek temporary relief of urgent symptoms by rational expedients, and either to encourage a buoyant anticipation of ultimate rescue by operation, or to lead the patient by degrees into the usual quiescence of confiding resignation to the inevitable, by the adoption of the tranquillising motto, *Cede Deo*.

“The local miseries which we have to alleviate mostly arise from pressure or congestion. The due action of the bowels and bladder are interfered with, the veins are pressed upon, and œdematous swelling of the extremities shows itself, the area of the chest is encroached upon and breathing is made difficult, a teasing cough supervenes from nerve irritation, or the heart is embarrassed and the brain action enfeebled. Common sense will suggest the fitting choice of sedatives or stimulants, aperients or enemas, the use of the cathéter, changes of position, the application of bandages or mechanical supports, and the possibility of relief sometimes to be obtained by manually shifting the position of the tumour when it is low down or impacted in the pelvis.

“The fact that vascular excitement and congestion aggravate every symptom, naturally enforces in a question of matrimony the Polichinellian advice, ‘Don’t;’ and as naturally enjoins, if not abstinence from, at any rate the utmost possible moderation in the use or permission of connubial rights. I shall have by-and-bye to discuss what must be done if pregnancy should unfortunately happen; at present I am content to urge that in all such cases caution should be given to avoid as much as possible the chances of it—*Concubitus sine Lucinâ*, if needs must.”

#### TAPPING.

It would not be possible to enter on the details of the surgical treatment advantageously here, but there are certain points that demand some notice—and, first, as to tapping. Professional opinion has been unsettled as to the use or propriety of this. Stilling tells us, “*No surgeon should ever puncture an ovarian cyst. Tapping is a crime;*” and, following him, Storer gives us the first of his “ten golden rules of ovariectomy:”—“*Never tap an ovarian cyst.*”



In the essay that obtained the gold medal of the Royal Academy of Medicine of Brussels, this question of tapping is very fully considered by the author, Dr. Gallez, as indeed is everything else connected with the "History of Ovarian Cysts," and the following abstract of the section on the "Results and Value of Tapping, and the Opinions of Authors," shows very fully the present state of our knowledge on the subject. In the first place Dr. Gallez quotes Stilling's denunciation of tapping as a crime, and gives the following as this surgeon's reasons for so viewing it. After each operation adhesions form; the constitution and general health are weakened; the volume of the tumour is increased; the removal of the tumour, if it should become necessary, is made more difficult, and consequently more dangerous to the patient; and, finally, tapping is useless, and never cures. And Stilling is not, says Dr. Gallez, alone in his advice.

2. After simple tapping patients die at least as soon as if they had not been operated on according to Sam Sharpe, Callisen, Sabatier, A. G. Richter. Berends, Marc, Hevin, Truckmüller, Hopfer, Martini, Delpech, Denman, and Burns, are opposed to the operation; and in the discussion at the Society of Medicine of Paris in 1844, Prus, Requin, Briquet opposed its adoption, and only admitted it in cases of absolute urgency, as Boyer had done before. Fock, by his statistics, has found that in 132 patients simple tapping once only brought about an amelioration or even almost a cure; as to the others it seemed to make their condition worse, 71 died in the course of the first year, and 25 of these in consequence, either directly or indirectly, of the operation.

3. Amongst modern ovariologists almost all are opposed to this palliative operation. Koeberlé admits it only when there is an absolute necessity, and when it is decided to undertake ovariectomy; successive tapplings, he says, in permitting us to temporize a little before attempting extirpation, allow, at the same time, the tumour to become worse, exhaust the patient, and render her less able to resist shock, hæmorrhage, peritonitis, and suppuration; they may be followed by death, and, in fact, cause a mortality much greater than ovariectomy practised in suitable cases, and only effect a cure in a very few and exceptional instances. If the liquid is brownish, bloody, we endanger the patients very seriously, and very uselessly; they become unfit for the operation, and often die miserably after the first tapping; the disposition to hæmorrhage from the vessels of the cyst, and their varicose condition are consequent on the

torsion of the pedicle. Ch. Clay, of Manchester, considers it as useless, dangerous, and only to be followed by evils.

For the same reasons, and, further, because the adjoining cells develop rapidly after the principal one has been tapped, and thus soon come to require the same treatment themselves, Ed. Martin rejects the operation; which, however, as we have seen, afforded him one success; and reserves it for those cases where there is a pressing indication for it, or where an injurious influence is exercised by the tumour on the general health.

Krassowski thinks tapping as dangerous as ovariectomy, and that it offers only the advantage of procuring a temporary relief. Let us see, nevertheless, says Dr. Gallez, on what basis the Russian surgeon founds such a dogmatic assertion:—

From 1858 till 1863 he says he has had under treatment 43 ovarian cysts, of which he considered 32 colloids, or multilocular, and 11 serous, or unilocular. Of the 32 colloids he treated 20 by tapping, abdominal or vaginal, 4 underwent it twice, 1 three times, and another four. There was a relapse after more or less time, 3 had peritonitis immediately after the operation; all died of exhaustion and suppuration. There is little importance to be attached to these results, for as we shall see further on, says Dr. Gallez, we ought to regard cysts of this kind as specially unsuited for tapping.

Of the 11 serous unilocular cysts six were treated by simple tapping.

In one case there was hæmorrhage after tapping (vaginal); plugging; convalescence extending over six months.

In one there was relapse after two tapplings; ovariectomy was practised. In two cases there was acute peritonitis which proved fatal.

Two had chronic peritonitis, with exhaustion and death.

4. Dr. Gallez next refers to the experience of Spencer Wells, as showing that tapping does not increase the mortality of ovariectomy, and that it may sometimes be a useful prelude to the removal of the tumour, and that the use of a syphon trochar reduces the risk to a minimum; but as in the work before us Mr. Wells gives his still more mature experience, we shall not quote Dr. Gallez's abstract of the earlier paper to which he refers. The results obtained by Southam are next referred to. These are much the same as those of Wells, but were only observed in seven cases. Gendrin is quoted as forbidding tapping, except in cases of

absolute necessity, and Pidoux as forbidding it on any terms whatever. "A la dernière extrémité ne faites rien, dit il, car les derniers moments de la malade n'appartiennent pas aux médecins."

5. Dr. Gallez has collected, from various sources, more than 600 cases, and tabulated them. Referring to this table, he says, we see, according to the statements of Southam, Kiwisch, and Lee, that 16 died after some hours, and 6 after some days—in fact, that 22 patients out of 131 (the number recorded by these authors) could accuse tapping as the direct cause of their death. We must make this reservation, however, that the condition of the patients at the time of the operation is not recorded.

On the other hand, it is right, says Dr. Gallez, to note that which is favourable to the operation. Out of the 131 cases there were 10 ameliorations and 3 radical cures—a result that speaks in favour of an operation regarded as a palliative.

6. Scanzoni, though but little of an advocate for tapping, admits that he has had two radical cures from it.

In the 400 cases of Wells and Velpeau, 6 only died after the operation, and yet Velpeau makes reservations. We shall see further on that it is these contra-indications that it is necessary to respect. The statistics of Barbosa, of Lisbon, are less re-assuring. According to him one-fourth of the patients die in consequence of the operation, a second fourth die in less than a year, and the remainder do not live beyond two to three years.

It is not rare, however, according to Velpeau, to meet women who had been tapped, for the first time, 10, 15, or 20 years before. Moreau tapped one woman 28 or 29 times, and saw another who had had it done 110 times, and observed a radical cure in another; and Dr. Gallez quotes more than twenty other authors, each of whom has recorded one or more cases of radical cure effected by tapping.

Dr. Gallez devotes sections of his chapter on tapping to the indications and contra-indications for the operation. The first question to be settled is as to the nature of the cyst to be tapped, and the period at which it should be done. If anything is to be hoped for from tapping, it is only when the cyst is unilocular, and its contents are serous. Demours and T. S. Lee recommend tapping as soon as possible after the diagnosis has been made, when the cyst is, as Trousseau has said, yet retractile, but the majority of authors recommend that it should not be done till absolutely necessary from the size of the tumour or the state of the general health. Dr. Gallez quotes Dubreuil, Franck, Moreau,

Kœberlé, Martin, Gendrin, Wm. Hunter, Camper, Morgagni, Boyer, Cruveilhier, Spencer Wells, and Scanzoni, as of this opinion, but he himself would prefer an early tapping, on theoretical grounds, mainly as establishing the diagnosis, and enabling a decision to be arrived at as to ulterior measures.

Peaslee examines the whole of this subject with great care, and shows that the danger of tapping polycystic ovarian tumours is so great that "at least one half of the patients die of a first tapping, while of oligocysts and monocysts alone, unless the operation is performed as a last resource, and when the patient is almost exhausted, the danger is very slight." As to the radical cures said to have been effected, he, however, asserts that when tapping cured, the disease was not ovarian, but probably a serous cyst of the broad ligament.

Wells thinks the danger of tapping has been greatly exaggerated. "As a rule," he says, "when a patient dies after tapping, it is not because tapping has hastened her death, but simply has not succeeded in saving her life." He does not mark the distinction between the tapping of polycysts and monocysts, but it is probable it is only in reference to the latter that he speaks. Previous tapings have not, in his experience, added to the risk of ovariectomy, and in some cases, unfortunately very rare, it has effected a cure. Of radical cures he gives the details of four cases. The first died about three years afterwards from some other disease, without any return of the tumour. The second was quite well at the end of seven years. In the third the tumour seems to have ruptured while the patient was dancing in June, 1860. In October, 1863, she began to get large again, and was tapped in July, 1865, and remained well till the end of 1866, when she began to re-fill. In the summer of 1867 she fell, and soon after profuse diuresis set in, and all swelling disappeared; and in 1872 Mr. Wells heard of her as continuing quite well. The fourth was tapped 25th March, 1865. The fluid was dark brown and viscid, but the tumour did not fill again, and in 1872 Mr. Wells had reason to believe she remained quite well.

We have ourselves seen two cases in which we have had reason to believe a single tapping was followed by a radical cure.

#### PREGNANCY COMPLICATED WITH AN OVARIAN TUMOUR.

The next question to which we would direct attention here is the conduct to be pursued when pregnancy is complicated with an



ovarian tumour. Dr. Peaslee does not express any opinion as to the treatment of these cases, though he refers to the cases of Wells, Atlee, and Marion Sims, and the discussions at the London Obstetrical Society. Gallez says, in the body of his essay, extirpation has not yet been undertaken on the living during labour,<sup>a</sup> but in the notes refers to Wells' cases and the discussions at the London and Dublin Obstetrical Societies, and to former Reports in this Journal. Wells gives the following rules as the result of his experience:—

“1. Pregnancy and ovarian disease may go on together, and may end in the birth of a living child and the safety of the mother.

“2. But in a large proportion of cases, probably in nearly all where an ovarian tumour is large, there is danger of abortion; or, if the pregnancy proceed to the full term, of lingering labour and a still-born child; and throughout the later months of pregnancy there is danger of sudden death to the mother from rupture of the cyst or rotation of its pedicle.

“3. Spontaneous premature labour may not save the mother from these perils, and the induction of premature labour artificially almost implies sacrifice of the child with considerable risk to the mother.

“4. There is no proof that tapping an ovarian cyst is more dangerous during pregnancy than at any other time; and if there be a large single cyst, tapping will afford immediate relief to distension at a very slight risk to the mother, and lead to the natural termination of pregnancy in the birth of a living child, if proper precautions be taken to prevent the escape of ovarian fluid into the peritoneal cavity, and the entrance of air into this cavity, and into the cavity of the cyst. In cases of multilocular cyst tapping can be of very little use.

“5. In cases of multilocular cyst, or solid tumour, the rule should be to remove the tumour in an early period of pregnancy; and if an ovarian cyst should burst during pregnancy at any period, removal of the cyst and complete cleansing of the peritoneal cavity may save the life of the mother, and pregnancy may go on to the full term.

“6. Of three cases on record where a pregnant uterus has been punctured during ovariectomy, the only recovery was in the one case where the uterus was emptied before the completion of the operation.”

It would be well, in speaking of ovarian tumours as complicating pregnancy and labour, to divide them into two classes—1. Those lying low in the abdomen, and likely to obstruct the passage of the

<sup>a</sup> L'extirpation n'a pas encore été entreprise sur le vivant pendant l'accouchement.

child through the pelvis; 2. Those lying high in the abdomen, and not interfering with the pelvis. Those lying low in the abdomen are always small, and not unfrequently solid, or semi-solid, in structure; they may be so small as to sink down completely into the cavity of the pelvis, or they may lie in, and more or less completely obstruct the brim, and in either case form a very serious complication to labour. Tumours such as these must, if labour occur, either be moved out of the way or lessened in size, or the child must be lessened in size or removed through other than the natural passages. As stated in a former "Report," and quoted thence by Dr. Gallez, Dr. Playfair has collected the details of 57 cases of labour complicated with tumours of this kind.

In 13 labour was completed by the natural efforts. 7 mothers recovered, and 6 died.

In 9 cases the contents of the tumour were drawn off by tapping. All the mothers and 6 children lived.

In 5 the tumour was pushed up above the brim, and all recovered.

In 15 craniotomy was performed. 8 recovered, but some imperfectly. 7 died.

In 5 version was performed. 4 died.

In 2 the forceps was used. 1 died.

It appears, then, from these statistics, that if a small tumour complicate pregnancy, and lie in the pelvis, and cannot be moved out of this position, or completely emptied by tapping, the propriety of its removal by operation ought to be considered as early in the pregnancy as possible, the alternatives being the induction of abortion or premature labour and Cæsarean section.

But if the tumour be high in the abdomen the same urgency does not arise. At the London Obstetrical Society, in December, 1869, Braxton Hicks related the particulars of ten cases, comprising eight pregnancies, where pregnancy was associated with ovarian tumours, but all of which were delivered at the full time of living children, without danger. And at the Dublin Obstetrical Society in March, 1870, we related, from our own experience, the histories of five pregnancies in two women where the same result followed, notwithstanding the presence of large ovarian tumours. Barnes has stated somewhat dogmatically, "that nature would not tolerate the concurrent progress of these two conditions. Nature could hardly bear the simultaneous growth of two tumours like the pregnant uterus and an ovarian tumour. Something must give way."

Such cases as those of Hicks and ours, and they are far from being alone, prove Barnes' assertion to be too hasty, and the rule may, we think, be safely laid down, that when pregnancy is complicated with an ovarian tumour lying high in the abdomen, interference is not demanded, unless the tumour, by its size and pressure on surrounding parts, interfere with the general health. If the tumour be unilocular, or nearly so, Wells has proved that it may be safely tapped, notwithstanding the pregnancy, if its size demand it. If the tumour be multilocular, or solid, Wells says it ought to be removed early in the pregnancy, but here, too, the size, position, and mobility of the tumour, ought to be taken into consideration before arriving at a definite conclusion.

#### USE OF DRAINAGE TUBE AFTER OVARIOTOMY.

The only further point to which it is possible to refer at present is the question raised by Marion Sims. The object of his paper is to prove that "septicæmia is the great outlet of life in ovariectomy, and to inquire what are the sources of this septicæmia, and what the treatment, prophylactic and curative." To prove his thesis Sims takes up the cases of death after operation, recorded by John Clay, Peaslee, and Wells, and seven *post-mortem* examinations he had himself seen. In all of these, he says, there was found a quantity of reddish serum, or grayish turbid serum, in the peritoneal cavity, and in cases slowly dying of pyæmia pyogenic reservoirs were found in the pelvic cavity. These pent-up fluids are, he says, the causes of the blood-poisoning that he believes to universally attend fatal cases of ovariectomy. To obviate this, Sims proposes to carry out still further the method of treatment originated by Peaslee, but the rule that he would establish for exceptional cases Sims wishes to make applicable to all alike. Instead of washing out the peritoneal cavity at the top, Sims proposes to open it at the bottom, as Peaslee did in 1855, and let the fluids run out spontaneously and continuously. He proposes to puncture the *cul-de-sac* of the vagina, behind the cervix uteri, and to pass a tube of some sort into the peritoneal cavity, to draw off any effusion that may take place in said cavity. He proposes to do this in every instance, whether there are adhesions or not. Peaslee, Wells, Keith, Kæberlé, have all done this where they had reason to think fluid had collected in Douglas's space, and often with most marked and beneficial results. Sims argues it is too late to do it when bad symptoms set in. During the operation of ovariectomy passing a

tube through Douglas's space into the vagina would be nothing. It would be very easily done, and would add nothing to the risk of the operation; but where the pulse jumps from 100 to 120, or 140, with vomiting, tympanites, hot skin, and oppressed breathing, it is too late to be operating in the dark on a dying patient's vaginal *cul-de-sac*. There can be no doubt this expedient of Marion Sims has much to recommend it besides his eloquent and enthusiastic advocacy of it, but we are not ready to admit it should be adopted in every case, as recommended by him.

It would not be right to close this Report without saying something as to the merits of the works on which it is founded. Wells and Peaslee's books are the most complete and exhaustive monographs we have ever read on this or perhaps any subject. They are both the result of great practical experience, great thought, great research, and great knowledge. No man should undertake the management of a case of ovarian disease who had not thoroughly studied both. Dr. Gallez has well won his spurs. His prize essay contains a thorough digest of all that has been written about ovarian cysts. Whoever is in difficulty, or thinks he has met with something new or rare, should consult its pages. As for Marion Sims' brief essay, it is like all he writes—clear, eloquent, enthusiastic; and while it carries the reader along, it convinces him that the teaching is good, and the practice worthy of adoption.



## PART IV.

### MEDICAL MISCELLANY.

---

*Reports, Transactions, and Scientific Intelligence.*

---

#### TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

*Wednesday, May 13th, 1874.*

SAMUEL GORDON, M.B., in the Chair.

*The Ophthalmoscopic Appearances of the Optic Nerve in Cases of Cerebral Tumour.* By C. E. FITZGERALD, M.D., M.Ch., Dubl.; Ophthalmic Surgeon to the Richmond Hospital; Assistant Surgeon to the National Eye and Ear Infirmary; Lecturer on Ophthalmic Surgery, Carmichael School of Medicine.

THE value of the ophthalmoscope, as an important aid in the diagnosis of cerebral and other diseases, is, at length, beginning to be recognised, though tardily, by physicians in this country. Hitherto this instrument has been looked upon as the almost exclusive property of the ophthalmic surgeon, but now there appears to be an anxiety on the part of the profession in general to learn something of its use. To inquire into the cause of this awakened interest with regard to ophthalmoscopy would be foreign to the present subject; suffice it to say, it exists, and should be earnestly cherished and encouraged by the specialist, for it must be remembered that the field of research to which the ophthalmoscope, in the hands of the physician, is adapted is very extensive, and, to prove of any real value, it is most important that investigations should be carried on *conjointly* by the physician and ophthalmic surgeon.

It is not my intention on the present occasion to indicate the various diseases in which the ophthalmoscope may prove useful to the physician and general practitioner. If the Society will permit, I hope to bring this subject under their consideration at some future meeting, yet I cannot forbear quoting the following passage from a paper read before the Massachusetts Medical Society by my friend, Dr. Joy Jeffries, of

Boston,<sup>a</sup> which illustrates very forcibly the value of the ophthalmoscope to the general practitioner. The author has just been speaking of some of the more abstruse and difficult points in connexion with ophthalmoscopy.

“But now,” he says, “aside from the nicer points in medical ophthalmoscopy, to be appreciated and used in diagnosis, perhaps still a long time, only by those familiar and expert with their presence and meaning—is the eye-mirror of as direct practical value to the general practising doctor as the microscope in determining vegetable parasites or urinary casts? I would reply most certainly, and especially to all at work away from centres of population, where till now ophthalmic surgeons alone find enough remunerative work, and to whom a patient may be readily sent. Let me here give a few examples in proof of my assertion. A patient awakes in the morning, seeing only the half, or a portion, of an object. This is a natural cause of fright to him and to the physician called in, who endeavours hurriedly to recall the cerebral troubles in which half vision occurs. At last, however, his wit is baffled, and the terrified man or woman is sent, perhaps, hundreds of miles by stage-coach, steam-boat, or cars to some ophthalmic surgeon. A glance with the eye-mirror shows him a partially detached retina, to become wholly so, very likely, by the necessary journey home. Surely, it would have been worth something to that village doctor, and still more to the patient, had the former been able to look into the globe, and diagnosticated what is as apparent as a torn or rumpled shirt-bosom—namely, separation of the retina.”

In these days of educational reform, when it is apparent we are on the eve of some important changes with regard to medical education, it might be worth considering whether a course of ophthalmoscopy should not form a necessary part of every medical man's study, at all events before he is admitted to the higher degrees. The undoubted services rendered by the ophthalmoscope, both in the study and practice of medicine, warrant the belief that the day is not far distant when the physician's education will be considered incomplete till he has mastered its use.

The ophthalmoscope appears to be specially adapted for assisting in the investigation of the large class of diseases known by the title of intracranial disorders, and the subject of the present paper has reference to what it reveals in cases of tumours situated within the cranium. I shall, first of all, describe the appearances presented by the optic nerve in cases of cerebral tumour, and then endeavour to give a *résumé* of the theories which are held regarding the mode in which the condition giving rise to these appearances is produced.

<sup>a</sup> The Value of the Ophthalmoscope to the General Practitioner. By B. Joy Jeffries, M.D., Harv. June, 1872.

The diagnosis of cerebral tumours is a subject which has always presented very considerable difficulties to the physician. Indeed, till within a comparatively recent period, any attempt at an accurate diagnosis of the presence of a cerebral tumour could scarcely be looked upon as much better than mere guessing. The study of this form of disease has, however, of late years received a great impetus, followed so far by such good results, that we are fully justified in expecting that the day is fast approaching when, not only the presence, but even the exact situation, of cerebral tumours will be diagnosed with as great accuracy as any of the well-known lesions of the lungs or heart.

Two circumstances account for the very remarkable advance made in this branch of medical diagnosis:—

1. The profound researches into the minute anatomy of the brain, especially with reference to the origins of the cranial nerves; more accurate observations with regard to the physiological functions of the latter, and the phenomena exhibited when the due exercise of those functions is in any way interfered with.

2. The discovery of the ophthalmoscope, and its application to the diagnosis of cerebral disease.

Von Graefe was the first to recognise and describe the peculiar appearances presented by the optic nerve in cases of cerebral tumour, and to offer a theory as to the cause of their production. He also very clearly laid down the features which distinguish these appearances from those presented by the other forms of neuritis optici. Unfortunately, subsequent observers have not sufficiently attended to these distinctions, hence some confusion has arisen, and a vast amount of valuable material has been wasted. For the future, however, any misapprehensions of this kind will be inexcusable, since such admirable works as, for instance, Dr. Allbutt's *Treatise on the Ophthalmoscope* place the necessary information within the reach of all who have already made themselves masters of that instrument. The following is the description given by Von Graefe of the ophthalmoscopic appearances presented by the first case which drew his attention to this important subject. It may be taken as a type of the condition of the optic nerve, which is generally, if not always, present in cases of cerebral tumour:—

“The optic papilla was considerably and irregularly swollen in such a manner that it rose abruptly on one side, and on the other sloped down almost imperceptibly to its ordinary level. The normal transparency of its tissue had disappeared, giving place to a greyish tint, with a very strong shade of red; the adjacent portions of the retina exhibited exactly the same change, so that the choroidal margin of the papilla was

\* On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys. By Thomas Clifford Allbutt, M.D. London, 1871.

hidden from view. The retinal opacity was uniform, presenting, nevertheless, with the erect image, a slightly striated aspect, which followed the course of the optic nerve fibres. The retinal veins were larger than normal, extremely tortuous, very dark coloured in parts, and issued in a very irregular manner from the opaque tissue; the arteries were relatively diminished in size. The opacity of the retina progressively decreased from the border of the papilla, forming, on the whole, a zone of 4 mm. in breadth, including, with the papilla, a circle of about 10 mm. in diameter.”<sup>a</sup>

Such is the description given by Von Graefe of this remarkable appearance of the optic nerve, and it only requires to be added that, in general, small hæmorrhages are found scattered over the papilla.

The patient in this instance died six weeks later, during an epileptiform attack, and the *post-mortem* revealed a very extensive sarcomatous tumour in the right hemisphere. Subsequent observations in precisely analogous cases led Von Graefe to the conclusion, that there was some relation between the tumour and this peculiar condition of the optic nerve, but, after very careful investigations, he finally decided that the latter was merely due to the increased intra-cranial pressure caused by the presence of the tumour. How he explains this we shall see further on. But though he thus concluded that there was no *direct* connexion between the tumour and the changes in the optic disk, he distinctly expresses the opinion<sup>b</sup> that the latter “plays an important rôle in the diagnosis of a cerebral tumour, inasmuch as where it is present in a very marked degree it is almost invariably accompanied by a tumour.” As I before mentioned, Von Graefe insisted upon there being two distinct forms of neuro-retinitis—the one, of which the description I have just given is an example, is characterised by remarkable prominence, redness, and loss of transparency of the disk, and this he looked upon as due to *mechanical hyperæmia*; the other, which generally accompanies encephalitis or encephalo-meningitis, he considered a *neuritis descendens*—i.e., an inflammation spreading from the brain.

“In this (the latter) form,” he says,<sup>c</sup> “the disk is also swollen, but not to the same extent, and it does not rise abruptly on one side; it is of a greyer colour, at the most of a reddish tinge; but it never presents the intense red of the other form. Moreover, the morbid process, which, as a rule, seems to develop more gradually, extends to a much greater distance beyond the disk, and attacks all the layers of the retina where it may manifest its presence by white patches, not to speak of numerous hæmorrhages.”

This form of neuro-retinitis need not occupy our attention further;

<sup>a</sup> Archiv für Ophthalmologie, 1860, Vol. VII., Part II., p. 58.

<sup>b</sup> Loc. cit.

<sup>c</sup> Loc. cit.



suffice it to say, that subsequent investigations appear to prove the truth of Von Graefe's supposition—namely, that in this latter form the whole length of the nerve is engaged in the inflammatory process, whereas in the former the neuritic process stops short at the lamina cribrosa.

The drawing (Plate VIII.) is of a case I have at present under observation, which exhibited this congested and prominent condition of the disk in a very marked degree.<sup>a</sup>

The history of the case is briefly as follows:—

J. C., aged twenty-nine, baker, married; was sent to me on the 6th of May, 1873, by an eminent physician of this city, who had examined his eyes with the ophthalmoscope, and diagnosed the condition of the disks.

The patient, a phlegmatic-looking individual, presented the appearance of a person suffering from great lassitude and fatigue. His countenance exhibited a curiously-pained expression, which forcibly recalled to my mind a picture the late Professor Robert Smith used to exhibit at his lectures, illustrating the peculiar expression of features presented by a person who had recovered from traumatic tetanus. There was well-marked paralysis of the external rectus of the left eye, together with very considerable anæsthesia of the whole of the left side of the head and face.

*History.*—He had always enjoyed good health until three months ago, when, for the first time, he experienced a very violent pain in the back of the head. After some time it ceased at this point, and attacked the left eye, and finally it shifted its position to the forehead. It was so severe that he could “neither eat nor drink.” He had no vomiting or sickness of stomach. For about seven months previously he had been drinking very hard, principally whiskey. The left eye began to turn inwards shortly after it had been attacked with the pain. He did not notice objects appearing double till about three weeks later. The whole of the left side of the head and face was “numb.” This numbness extended exactly to the mesial line. The cornea might be touched without producing any reflex movements. He experienced considerable difficulty in opening his mouth to any great extent, as “the jaw on the left side felt as if it were locked.” He complained of great lassitude and depression, and that he could not walk for any distance without wishing to sit down. At home he “would like to be lying down all day,” though were he to do so he could not sleep; he slept badly at night; he had always to lie on the left side; were he to lie on the right side, or on the back, he immediately experienced intense pain in the head or in the left eye. There was no history of syphilis.

<sup>a</sup> For other drawings illustrative of this condition of the disk, see Liebreich's *Atlas d'Ophthalmoscopie*, 2nd Ed., Pl. XI., Fig. 2, and Magnus's *Ophthalmoscopischer Atlas*, Pl. III., Fig. 8, Leipzig, 1872.





On testing his vision I found he could read XX. (Snellen) at 15'. With the ophthalmoscope both eyes presented almost similar appearances; the disks were enormously congested, and very prominent; the margins ill-defined and hazy; the veins were greatly engorged and tortuous, and presented well-marked curves at the edges of the swollen disks. Owing to the zone of effusion surrounding the margin of the disks, the veins were veiled, or were completely hidden for some distance; the arteries were greatly reduced in size and number, the few that were visible being quite small and thread-like.

The treatment consisted in the administration of five grain doses of the iodide of potassium three times daily. After continuing this treatment for some time, the patient expressed himself as greatly better, and indeed there was a marked improvement in his appearance. His countenance had lost the peculiarly pained expression to a great extent, and, on the whole, he looked much brighter; he slept much better now. The bromide of potassium was now combined with the iodide.

The following month feeling had returned to the left side of the head and face, with the exception of one small portion of the lower lip. He continued to improve rapidly, and with the ophthalmoscope the veins could be traced much more distinctly at the margins of the disks. He was now able to resume his work, and stated that he felt almost as well as ever.

Towards the end of last August he again presented himself. His countenance exhibited the same peculiarly pained expression that it did at first, and he stated that for the last few weeks he had suffered greatly from the frontal pain. He was obliged to give up working, and he would "fain" be constantly lying in bed. The left pupil was dilated, but responded to light. No change could be detected in the condition of the disks, and vision remained the same. His memory was very defective; he forgot my name a few minutes after he was told it.

I now lost sight of the patient for about two months, and when I next saw him he was an inmate of one of the general hospitals in this city, where he had been admitted with well-marked exophthalmos of the left eye, and ptosis of the left eye-lid. I then examined him with the ophthalmoscope, but could detect little or no change in the disks—perhaps they were not quite so prominent.

I saw nothing more of the patient until December, when he again came to me. He had been suffering intense pain across the forehead, the left side of the head and face, and in the left eye. The exophthalmos and ptosis had greatly diminished.

He was admitted into the South Union Workhouse, where I attended him. A subcutaneous injection of morphia relieved the pain somewhat. He was ordered the iodide of potassium; in a few days the pain lessened, and in about a week he left the Workhouse. The exophthalmos and



ptosis gradually subsided. The patient now returned to work, but still continued taking the iodide of potassium, and visited me regularly. By-and-bye I noticed a slight droop in the right eye-lid, which increased rather rapidly until there was well-marked ptosis. The patient did not complain of any pain, but, on the contrary, stated that he felt perfectly well. *Pari passu* with the ptosis, the right oculomotorius became almost completely paralysed. The swelling and prominence of the disks had subsided, and both optic nerves showed signs of atrophy. The patient continued taking the iodide of potassium, and was able to pursue his employment.

On the 24th of March his wife brought him to the National Eye and Ear Infirmary. She stated that four days previously she noticed a strangeness in his manner; at one moment he would be cheerful and even boisterous, and then shortly afterwards he would become morose, and inclined to quarrel. She attributed it to his having gone out that day with a friend, and partaken of some liquor. I should mention that, from the first, I had forbidden him to take any stimulants, and I have every reason to believe that, up to this date, he obeyed my injunctions.

On examining him I found him greatly changed; he looked heavy and stupid; he complained of great pain in the right eye; his memory was defective, for he had forgotten my name, though he recollected the day of the week, &c.

He was again admitted into the South Dublin Union, and the same evening, it appears, he was rather violent, and had to be restrained. Subsequently he became quiet, and remained so the rest of the time he stayed in the Workhouse. He had frequent fits of crying—in fact, if he saw any one with whom he had formerly been acquainted, he was immediately moved to tears. I prescribed the iodide of potassium, finding it of such service before.

On the 30th of last month (April) his wife came to me, and stated that on visiting him that morning she had found him very ill; that he had lost the power of one side, and could not speak. I saw him the same day, and found him hemiplegic on the right side; his speech was quite thick and unintelligible, and he was frequently crying. The wardman stated that he found him in this condition in the morning when he went to get him out of bed. Contrary to my wishes, his wife insisted on taking him home. I have seen him twice since; he is regaining some power over the leg and arm, and his speech is also improved.

Such is the history of this very interesting case. I am fully satisfied in my own mind that it is one of tumour in the brain, for the symptoms warrant such a conclusion, and the appearance of the optic nerve is typical. At the same time I find very considerable difficulty in arriving at any very definite conclusion as to the locality of the tumour.

I now pass on to consider briefly the theories which have been put forward to account for this congested condition of the disk in cases of cerebral tumour; and first in order stands that proposed by Von Graefe. According to him, pressure upon the cavernous sinus is the starting-point of the process. "This pressure," he says,<sup>a</sup> "necessarily produces a stasis in the retinal veins, which become very large and tortuous. The swelling of the disk from serous effusion and hypertrophy of the cellular tissue, which in course of time follows, is also very easily accounted for by this mechanical hyperæmia."

Another very important factor in the production of these conditions, and one which Graefe lays great stress on, is the unyielding sclerotic ring, which, as he expresses it, "acts the part of a multiplier."

This is the explanation given by Von Graefe as to the mode in which the *Stauungs Papilla*, as he terms it, is produced, and greatly modified, as it has been lately, by the investigations of Schwalbe and Schmidt, the "pressure theory," if I may so term it, still counts the largest number of supporters. The theory which stands next in chronological order is that propounded by Benedict.<sup>b</sup> He looks upon the neuro-retinitis as due to a morbid innervation of the sympathetic, which is itself a symptom of the most varied cerebral diseases. A full account of this theory may be found in Dr. Allbutt's admirable work, already referred to, where it is also criticised in a most able and masterly manner.<sup>c</sup>

Dr. Hermann Pagenstecher, who is an upholder of Benedict's theory, in an interesting paper on Optic Neuritis,<sup>d</sup> published in the London Ophthalmic Hospital Reports, enumerates the following objections against Graefe's theory:—

"1. The cavernous sinus is a blood canal surrounded with such impervious walls as are rarely to be met with, and a pressure sufficient to really compress this must, since the pressure within the cranium must be regarded as equal in all directions, exercise enormous pressure on all the other blood-vessels."

"2. The vena ophthalmica superior, and vena ophthalmica inferior anastomose extensively, both with one another and also with the facial vein. And although the vena centralis retinae empties itself almost immediately into the cavernous sinus, yet it previously anastomoses freely with the vena ophthalmica superior. But besides these anastomoses, it is quite indifferent whether the discharge takes place at the vena ophthalmica or into the cavernous sinus, since both are continuous, and the return of the blood suffers no hinderance so long as the way through the facial vein is patent."

<sup>a</sup> Loc. cit.

<sup>b</sup> Elektrotherapie, Wien., 1868, p. 249, et seq.

<sup>c</sup> Op. cit., p. 119.

<sup>d</sup> The Royal London Ophthalmic Hospital Reports, Vol. vii., Pr. ii., p. 160.

This objection was first raised by Sesemann, who has written a very interesting paper on the orbital veins, in Du Bois Reymond's Archives.<sup>a</sup> I shall merely quote one passage from it:—

"I am decidedly of the opinion," he writes, "as the result of my investigations, that in most cases the blood from the ophthalmic vein flows both into the sinus and into the facial; but, indeed, I think that by far the greater part empties itself into the latter. I arrived at this conclusion from the fact that the diameter of all the openings into the facial is greater than that into the sinus cavernosus, which is sometimes so small that only a very little of the blood can find its way through it. . . . In my opinion the inferior ophthalmica vein serves not only to carry away the blood out of the other orbital veins, but also serves as an outlet, a so-called 'emissary vein' of Santorini for the sinus cavernosus. All the other sinuses have such emissaries, why should not the ophthalmic have the same relation to the sinus cavernosus? The emissaries, as a rule, empty themselves into the sinus, and it is only when the pressure in the latter is increased above what it is in the other veins connected with it, that the blood finds its way through them. This is what occurs in the ophthalmic, as much blood as can make its way through the narrow opening into the sinus does so, the rest flows into the anterior facial. But should the pressure in the sinus, through any circumstance, be increased, the ophthalmic not only discharges its contents into the facial, but, moreover, carries off the blood from the sinus."

If Sesemann's investigations be accepted, they must prove fatal to the "pressure theory," as expounded by Von Graefe, and indeed they appear to be generally viewed in this light by most recent writers on the subject.

To return to Dr. Pagenstecher's objections.

"3. Not every increase of cerebral pressure is followed by neuritis, and, undoubtedly, neuritis occurs when there is no such increased pressure.

"4. Optic neuritis is sometimes one-sided, and, indeed, has been often observed on the side opposite to the cerebral lesion, whilst the eye on the same side has remained perfectly normal."

The latest theory is that advocated by Schmidt, and is based upon some experiments he has made (similar to Schwalbe's) of injecting fluid into the arachnoid space.<sup>b</sup> He asserts that the injection fills the intravaginal space of the optic nerve, and that it "then empties itself into a 'canal system' which ramifies in the lamina cribrosa. He concludes therefrom that the *Stauungs Papilla* arises from increased intra-ocular pressure, due to *Stauung* of the injected fluid in the canal system continuous with the arachnoid cavity."<sup>c</sup>

<sup>a</sup> Archiv. f. Anat. u. Phys., 1869, No. 2.

<sup>b</sup> Archiv. f. Oph., Vol. xv., Pt. ii., pp. 193, 197.

<sup>c</sup> Allbutt. Op. cit., p. 57.

This theory has very recently been called in question by Dr. Forlanini, in the Italian Annals of Ophthalmology.<sup>a</sup> Dr. Forlanini has repeated Schmidt's experiments frequently, but without success.

Pagenstecher says,<sup>b</sup> "one would be inclined to accept this theory as soon as one became convinced of the existence of such a system of canals in the lamina cribrosa as are presupposed;" but, he adds, that hitherto he has been unable to demonstrate it.

Such are the theories held regarding the mode in which the Stauungs Papilla is produced. It will be seen that the question is far from being definitely settled yet. Perhaps I ought to mention one practical point in connexion with the subject, which may possibly be urged in favour of Schmidt's theory. At the meeting of the International Ophthalmological Congress, held in London the year before last, Dr. de Wecker, of Paris, read a paper on an operation he had devised for the treatment of neuro-retinitis. It consists in making an incision in the sheath of the optic nerve and sclerotic ring, and so giving exit to the contained fluid. He had performed this in only two cases, both of them supposed to be cases of tumour in the cerebrum. The neuro-retinitis was regressive in both cases. Nevertheless though there was little or no improvement in the vision, the intense headache was in one case relieved; and in the other case the patient, who, before the operation, was affected with weakness of the legs, was afterwards able to stand more firmly and answer questions promptly. He appeared greatly pleased with the result himself. An account of the operation will be found in the report of the Congress.<sup>c</sup>

In the last volume of the St. Bartholomew Hospital Reports, Mr. Power records an interesting case of optic neuritis,<sup>d</sup> in which he performed this operation. He states, that "it seemed to relieve the pain, which was intense."

In concluding, I would wish to correct a rather widely spread, but most erroneous impression, which is this—that Von Graefe regarded the Stauungs Papilla as *absolutely* diagnostic of the presence of a cerebral tumour. Such is not the case. Graefe looked upon this condition of the optic disk as merely the *expression* of increased intra-cranial pressure, and, consequently, that whatever caused an increase of the pressure would also produce this swollen state of the disk. That it may be present in cases where there is no intra-cranial tumour there is no possible doubt, and Graefe did not deny it. This drawing, which I made from a case under Dr. Gordon's care, exhibits the prominence and swelling of the disk in a very high degree; yet, I believe there is no

<sup>a</sup> Annali di Ottalmologia. Pt. I. 1871.

<sup>b</sup> Loc. cit.

<sup>c</sup> Report of the Fourth International Ophthalmological Congress, held in London, August 1872, p. 11.

<sup>d</sup> St. Bartholomew's Hospital Reports. Vol. IX. 1873. p. 196.



possible doubt that this was a case of very severe meningitis. The patient, a young girl, was in a semi-comatose state when I made this drawing. She recovered, but the sight is gone. There is complete atrophy of both optic nerves.

I am aware that an objection may here be urged to the effect, that if the same appearance can be produced by a cause other than tumour of the brain, such appearance is after all of very little practical value as a diagnostic sign. To this I would answer, that apart from a most careful study of all the symptoms in any particular case, it can scarcely be looked upon as of any special diagnostic value; but that, on the other hand, due regard being paid to those symptoms, it must prove of invaluable assistance in forming a diagnosis. "My own opinion certainly is," says Dr. Allbutt,<sup>a</sup> "that changes either of a congestive, neuritic, or atrophic character may be found in the disks, at some time or other, in the course of almost all cases of intra-cranial tumour. The diagnosis of a case of this kind is therefore incomplete unless the eye-mirror has been carefully and repeatedly used."

The CHAIRMAN said they would all acknowledge the great value of Dr. Fitzgerald's paper, especially as regards its illustration of the means of diagnosis given by the ophthalmoscope. Several of the questions referred to by Dr. Fitzgerald were of importance, but the points he had dwelt upon as indicating the presence of cerebral tumours, were of great value. With regard to the particular case under his (Dr. Gordon's) care, to which Dr. Fitzgerald had alluded, it was a very remarkable example of intense cerebro-spinal meningitis, in which the cerebral symptoms prevailed over the spinal centres. He did not think he had ever seen a case in which there was such intense agonising pain, lasting for days, as there was in that case—only relieved by intense cold, a large amount of local depletion, and iodide of potassium in large quantities. The girl recovered, but with complete loss of sight.

DR. STEWART expressed his gratification at hearing Dr. Fitzgerald's able and learned paper. His attention had been for many years directed to the treatment of the insane, and he hoped from what Dr. Fitzgerald had stated, that the ophthalmoscope might be found to throw some light on the mysterious diseases of the brain.

DR. H. KENNEDY said that several papers had recently appeared in the medical journals with reference to the use of the ophthalmoscope in cases of the insane. With reference to some points in Dr. Fitzgerald's admirable paper, he would refer to the first case detailed. It came within the range of possibility that it might have been of a syphilitic

<sup>a</sup> Op. cit., p. 118.

nature. The effect of the iodide of potassium, though given in small doses of five grains, he believed, was so decisive as to favour that view. An English physician lately published a case in which he gave twenty grain doses, and he said that, as a general rule, a smaller dose would not affect the case.

MR. B. WILLS RICHARDSON said that there was a drawing in Dalton's Physiology illustrative of the effects of division of the right sympathetic cord in the neck of a cat, which seemed to bear upon one of the theories mentioned by Dr. Fitzgerald in his very valuable communication, for not only was there produced contraction of the pupil from paralysis of the radiating fibres of the iris in the cat operated upon, but there was also paralysis of the levator palpebræ superioris muscle, and the third eye-lid, or nictitating membrane, was drawn over nearly the whole cornea. Dalton attributes these effects to an exaggerated sensibility of the retina, caused by vascular congestion, the ptosis and contraction of the pupil being a consequence of the congestion.

There was another point alluded to by Dr. Fitzgerald, and illustrated in the very excellent drawing he had exhibited—namely, the free communication between the facial and the orbital veins. It had been observed by some practical writers on anthrax that, when it implicated the face, death from blood-poisoning was not an infrequent result, and that in some of those cases pus may be found in the cavernous sinus, which they attribute to the spreading upwards of the so-called phlebitis of the facial vein, and thence to the orbital veins. In cases of this kind rapid destruction of the eye-ball had happened, which probably originated in embolic plugging of some of its veins. A case of that description occurred a few years ago in the Adelaide Hospital. The anthrax engaged the chin and the lower lip. Rapid destruction of one of the eye-balls followed, and eventually the patient (a woman) died with well marked symptoms of blood-poisoning.

*On Intestinal Hæmorrhage in Fevers.* By HENRY KENNEDY, A.B., M.B.; Vice-President, College of Physicians; Physician to the Whitworth Hospital, Drumcondra, &c.

IN the following paper I would ask attention to the occurrence of intestinal hæmorrhage in fevers. This subject has come into my mind in consequence of my having read one of the valuable Lectures just published by Dr. Stokes; and, I would add, so ably edited by my friend, Dr. J. W. Moore. The Lecture to which I allude contains the remark that, important as the separation of the different types of fever is, it is still more important that the points in which they agree should be ascertained and recognised. It is obvious that the more general the resemblance

of the symptoms of disease, known to be closely related to each other, are, and fevers come under this class—it is obvious, I say, that the treatment will be the more definite and fixed. Thus, taking the symptoms of which I am about to speak as an example, it is plain that the more there is known about hæmorrhage from the bowels, and the varied circumstances under which it occurs in fevers, cannot but affect, and in a very important way, both our prognosis and treatment. My impression is that, on this special point, the views commonly held are too exclusive, just as I believe them to be on the vexed question of the differences of typhus and typhoid fevers; and before these remarks are concluded, I hope to prove the former to the satisfaction of the meeting.

Independent, however, of these considerations, intestinal hæmorrhage is ever a symptom which must arrest our attention. It is always a grave occurrence, and too frequently has a fatal ending. For these reasons, then, this subject seems to me worthy the notice of the Association, and I now proceed to make a few remarks on it, and so add the additional experience of some years to my former paper, for one was published on the same point as far back as 1855.

Before, however, entering on the immediate subject of this paper, there are two remarks of a general character which I wish to make; the first is this, that there is a tendency in some constitutions to bleedings—spontaneous bleedings, if we may call them, and this without any special diathesis to account for them. There are, I presume, few present who have not met such cases, whether from the nose, chest, lungs, or bowels. Now, this is a fact which should not be forgotten. It is obvious, that if fever attack any such cases, the tendency to hæmorrhage will be much greater than if such did not exist. It is plain, too, if bleeding occur in any of them, it is not necessarily connected with any specific type of fever, such as typhoid. Before I end these remarks, I hope to prove this point.

The second general remark I would make is, that hæmorrhages of all kinds are much more common in some seasons than in others. Long since I made the remark as regards epistaxis, which is almost certain to occur if the weather be very warm, and I think I may now say the same about bleeding from the bowels, not that it occurs in warm weather, but that it is much more frequent at some periods than others. It need scarcely be added that this unusual prevalence of cases of a particular type, and at the same time, is not confined to hæmorrhagic cases. It is of importance, however, that the possibility of such an occurrence should be kept in mind. With these brief remarks I shall now proceed to the immediate object of this paper; and here it becomes necessary to make some division of my subject, and for this purpose I shall speak of intestinal hæmorrhage in fever as being met under three distinct phases, if I may so describe them. First, when the bleeding occurs in early life, from seven to

fifteen years of age; secondly, when it is met in typhus fever; and lastly, when it complicates typhoid fever. In the first of these—that is, where bleeding occurs in early life—it has always been a late symptom of the fever. Fifteen to twenty days have usually passed over before it appears, and it may recur again and again, and yet the patient may ultimately recover. In connexion with it, the type of the fever is particularly worthy of note, for it is not of the typhoid type, as might, *a priori*, be supposed, but what I have been in the habit of calling gastric. Of course I admit that typhoid fever exists in children and young people, and may then be complicated with bleeding from the bowels, but that bleeding will occur in these cases without any ulceration or affection of Peyer's patches is certain. The rapidity with which some of these cases recover after hæmorrhage is remarkable, and quite opposed to the idea that it rose from, or was in connexion with, ulceration. Independent of this, however, *post-mortem* examination has proved that bleeding will occur without any lesion to account for it. I had been aware of this fact, as occurring in the course of typhus fever in the adult, before I knew of its possible occurrences in childhood, but the following case, which came under my notice some years since, set the matter at rest; and, I would add, the case itself is, in other points of view, one of peculiar interest and importance.

CASE I.—In October, 1852, a girl of thirteen years of age was admitted into the Cork-street Hospital, labouring under fever. She had been sent from the South Dublin Union Workhouse, and was about one week ill. Her fever presented no peculiar feature, and matters went on till she was either twelve or thirteen days ill. On the evening of the thirteenth day she was suddenly seized with collapse, marked by cold and livid extremities, and very weak pulse at the wrist; she also vomited, and complained of pain in the abdomen: but this was not increased by pressure. She lay on her back with her knees drawn up, and was perfectly collected. She continued to vomit, or rather to retch, at intervals through the night, and occasionally complained of the pain, but neither of these was of a severe character. Her face, however, assumed a more pinched and livid look, her extremities got colder and colder, the pulse failed at the wrists, and she died at nine the next morning, just fourteen hours after the signs of collapse had set in. An enema she had got had only brought away a small quantity of fæces. Being unable to get an examination of the body at the hospital, I followed it to the South Union, where, through the kindness of my friend, Dr. Shannon, I was able to do so. The body was fully grown for a girl of her years, and there were slight marks of cicatrices on one side of the neck. The head was not examined, and the chest was, in every respect, healthy. The abdomen was scarcely opened till the cause of death became at once apparent. A



large portion of intestine was seen to be filled with blood; and, on closer examination, this proved to be the lower part of the jejunum and upper part of the ileum. About twenty inches was so engaged. The blood lay here loosely coagulated, and of a very dark colour. The corresponding mucous membrane was stained of the deepest hue; but the most minute examination could not make even a trace of ulceration, either where the blood lay, or anywhere else. There was no trace of blood either above or below the part indicated; near the sigmoid flexure lay a small portion of fæces of the natural colour. The lower portion of the ileum was healthy, and the mesenteric glands were not enlarged at all. Judging from the state I found the blood in, it seemed as if the process of pouring it out had been a gradual one, probably occupying from the commencement of the signs of collapse till the death of the patient.

Though this case is a solitary one, it is quite sufficient to prove the fact that hæmorrhage, even to a fatal extent, may occur, and this without any ulceration whatever, or, what is of more consequence, without any implication of Peyer's patches. Murchison speaks of having different times met cases where the blood was retained within the bowel. But he does not seem to me to have noticed what is really the important fact—viz., that the bleeding may occur without any ulceration. I have little doubt I have seen other cases exactly similar to the one detailed, for I have notes of cases where sudden death occurred, which, however, wanted the absolute proof that a *post-mortem* examination afforded. Besides, in most of the cases, though bleeding occurred and came outwardly, the patients recovered, and the recovery was so rapid that I could not bring myself to believe ulceration had existed, for the healing of ulcers is, we know, under such circumstances, a very slow process; and, indeed, my present conviction is, that in the majority, at least, of the cases the hæmorrhage was not in connexion with ulceration. It seems to me, I repeat, that this is a very important point in the natural history of these fevers.

Speaking generally of the bleedings which occur in early life, I think they are more apt to be repeated at this age than in the adult; at least this has been my experience. I have known them recur several times, even at intervals of a week, and yet the patients will recover. Now, this is not what occurs in the adult, for if the bleeding recur with them the intervals are very much shorter, and the shock much more marked. It has been matter of observation to me, too, that these bleedings in the young, if they do not occur in strumous, are, at least, met in what must be considered delicate constitutions.

The second form of intestinal hæmorrhage of which I would speak is when it occurs in typhus fever. On this point I am not aware that I can refer to any author, for it would seem to be assumed that bleeding in this type of fever is unknown. In the elaborate and, I would add, able work

of Murchison, I can find no allusion to it, and yet it does occur, and should, of course, be recognised. Possibly the detail of a few cases will not be out of place here.

CASE I.—Byrne, a man aged thirty-five, was admitted into hospital in March, 1850. He was the eldest of three brothers, all in the hospital at the same time, and affected with typhus, being spotted. This patient was two days ill before the others, and his fever was of a very urgent character, his chest being much engaged. The pulse was 130, and his brain heavy and confused. On the morning of the seventeenth day, as nearly as could be made out, he was suddenly seized with bleeding from the bowels to the amount of twelve ounces. The blood was very nearly pure. Immediately after it was reported he had some kind of fit. When visited his hands were livid and cold, his breathing very hurried, and his pulse could not be felt at the wrist. It is enough to say of this most unpromising case that he finally recovered.

CASE II.—In June, 1855, a man of twenty-seven years of age was admitted into hospital, on the sixth day of his fever. Spots were already visible, and in the course of the next two days he exhibited a copious crop of petechiæ. The illness was very severe, the brain being much engaged. His eyes were deeply suffused, and the pupils contracted. There was constant raving, and a person was required to be with him night and day, as he was ever striving to get out of bed, and even out of the ward. The pulse ranged about 112, and was on some days dicrotous, and he put out his tongue badly. Though as active measures as the case allowed were put in force, still the fever did not yield. At last, on the night of the eleventh day, he was suddenly seized with bleeding from the bowels, and passed, at two turns, about 12 ounces of blood, some of it in clots. This caused but little shock, except that in the morning his face was a shade paler than the day before. But it did cause a very marked improvement in the fever, and though it could not be called critical, as the fever went on a few days longer, still there was no fear afterwards for the man's life, which there had been previously, and the patient made a good, and even rapid, recovery.

CASE III.—A man of thirty-five years of age was admitted in an advanced stage of fever. He was densely spotted, and had constant raving, whilst his pulse was very rapid, and running together. In this state he was seized with copious hæmorrhage from the bowels, which recurred again and again, and he rapidly sank. This patient had been kept in a wretched room, and nothing had been done for him.

CASE IV.—A woman of fifty years of age was admitted, labouring

under heavy fever, which had all the characters of typhus; but the colour of her skin was so dark that anything of spots could not be made out. Her aspect, however, was very marked, and the eyes deeply suffused. The chest was most seriously engaged with what appeared to be suffocative catarrh. The duration of her illness could not be made out, but it was known she was some days ill. Whilst in this state, and to all appearance *in extremis*, she began suddenly to pass blood, nearly pure, from the bowels. This, so far from injuring her, relieved the chest. The dyspnœa, which had been most urgent, lessened, and with it the other symptoms improved, and this apparently hopeless case recovered, though after a severe struggle.

The cases detailed are quite sufficient to prove that the type of fever known as typhus may be attended with bleeding from the bowels. Besides these, however, I may allude to two others where I had an opportunity of making examinations after death, and in which during life intestinal hæmorrhage had occurred. Several days had elapsed between this occurrence and the death of the patients, and when I came to examine the intestines, and look for the source of the bleeding, I confess I was surprised at being unable to make out where the blood had come from, for not a trace of any lesion could I find, or blood either, though looked for most closely. Here then was additional evidence, if such were wanting, that hæmorrhage may occur, in the course of typhus, from the bowels, and that, under such circumstances, the bleeding does not arise from ulceration. Hence these cases are quite analogous to those already spoken of as occurring in childhood.

The last class of hæmorrhages of which I would now speak are those where the bleeding occurs in the progress of enteric fever, and which, I need scarcely add, are best known to us all. On this particular class I have nothing particular to say. It would seem to prevail in some countries more than others. Thus, whilst it is rare in America and France, it has been met in London, according to Jenner, in every third case. But what is of more moment, these hæmorrhages prevail much more at one period than another. Thus, during the past winter, the members of the Pathological Society will recollect the number of cases of enteric fever brought forward, in very nearly all of which death occurred from intestinal hæmorrhage. Drs. Stokes, Foot, Hayden, Moore, Nixon, and others all brought cases, and some of these gentlemen more than one example of the same kind. It is particularly worthy of note, that in some of these neither the temperature, nor any other symptom, would have led to the idea that a fatal hæmorrhage was about to occur. In fact, they were, in the strictest sense, insidious cases, and quite capable of throwing the most experienced off their guard. The quantity of blood, too, thrown out was very remarkable. As to its exact source, I

believe it is more in keeping with facts to suppose that it came from a violent congestion of the mucous membrane, than from a vessel opened by the ulcerative process. Of course, this latter may, and does, occur; but, after what has been brought forward this evening, it seems to me the former idea has the most facts in its favour. If the ulcers of enteric fever were the source of the bleeding which occurs, it strikes me it would be a much more frequent symptom than it is known to be, for we must keep in mind that it is, in reality, a rare complication of the fever. This point of the source of the bleeding is not one of mere curiosity, for it is obvious that, according to the ideas we hold, our treatment will be modified accordingly, and a special medicine, such as turpentine, which might suit well one form, would, on the contrary, be unsuited for the other. This point, then, is one which I think worthy of a more special attention than it has yet got.

As to the question of the mortality of these cases of intestinal hæmorrhage, I quite concur with those who consider it rather favourable than otherwise. Many years have elapsed since the late Dr. Graves announced this opinion, and in Trousseau's great work he expressly quotes Graves as being the first to have drawn attention to the point. Under my own eye the mortality has been, as near as may be, one in four, and I have now tabulated very nearly fifty cases; but I am not able to give as exactly as could be desired the precise numbers of the different types, for many of these were noted without any reference to the special type of fever present. It is something, however, to know that, taking them all round, the mortality was about one in four. I have the impression, too, that cases in private are more fatal than those in hospital practice, just as we know occurs in typhus fever, where the mortality is so very much greater amongst the middle and upper ranks than with those cases seen in an hospital. The mortality of enteric fever generally, too, would appear to be higher in some places than in others. Thus, in London, Murchison makes it as high as 17 per cent. I believe it, as typhus certainly is much more fatal there than with us in Dublin, and, like other diseases, to be far more serious some years than others.

There is one symptom in connexion with hæmorrhage on which I would wish to make a remark. I mean a dicrotous pulse. Speaking generally of it, it does go hand-in-hand with bleeding, and it often precedes it, and so constitutes a useful warning. But I have seen a number of cases where the pulse was dicrotous, even in a marked degree, and yet no bleeding occurred, so that, like any other single symptom, it must be taken with qualifications, just as a high temperature is.

In concluding these remarks, which may well be called "*dissecta membra*," I would observe that I have had three principal points in view:—the first being that, in accordance with the valuable observation of Dr. Stokes, it is of much consequence that the points on which the different



types of fever agree with each other should be known and recognised, and that, as a corollary from this, intestinal hæmorrhage is common to the different types, such as typhus and enteric fevers. In the second place, I wished to establish the fact, that this form of bleeding occurs both in the young and in adult life, without any ulceration of the mucous membrane, or affection of Peyer's patches. And lastly, whilst I admit that the bleeding in a case of typhoid fever may, and does sometimes, arise from an open vessel, there are good grounds for believing that it may oftener arise from a general exudation of the mucous membrane. And, finally, should this last view prove to be correct, it would modify materially the class of medicines to be used against this accident.

DR. DARBY said that, having had charge of a large fever hospital for many years, he did not like to allow this comprehensive and suggestive paper to pass unnoticed. In the year 1848 he had a run of cases admitted into the fever sheds, which were built in addition to the permanent hospital he had charge of; and on that occasion he, for the first time, saw hæmorrhage in connexion with typhus fever. The cases, strange to say, were of a different form from the fever he saw in the Dublin hospitals at that time, which were mostly of a typhoid character, while he had seen genuine cases of maculated fever—some complicated with jaundice. He would not call it yellow fever, although the discoloration of the skin was so great as to justify the term, but there was no black vomit. In some of these there was hæmorrhage, which at first greatly alarmed him. The first was that of a stout man, about thirty years of age, who came in with maculated typhus and jaundice. About the thirteenth or fourteenth day he had extensive hæmorrhage from the bowels. He (Dr. Darby) made up his mind that the man was about to die, but, on the contrary, the fever left him, and he got well immediately. The hæmorrhage seemed to be critical. He had eight or nine cases of the same kind within six weeks or two months, all terminating in the same way, with, as he believed, a critical hæmorrhage. He did not remember ever having seen a case where there was hæmorrhage from the bowels in the latter stage of typhus fever that terminated fatally. He had seen one fatal case of hæmorrhage from the bowels in a case of typhoid, which, he believed, was the result of ulceration. It occurred in a child nine or ten years old, who came in with advanced typhoid fever, and had repeated attacks of bleeding from the bowels. There was a *post-mortem* examination, which satisfied him that the bleeding came from the ulcers. He should be afraid of hæmorrhage in an advanced state of typhoid fever; but, as far as his experience went, hæmorrhage in cases of typhus was rare, and not so dangerous as he at first apprehended. The eight or nine cases he had referred to as occurring in the end of 1848, or the

beginning of 1849, were, as he believed, true cases of typhus, maculated, and with low muttering delirium, &c., and they all recovered. He had not seen any case of hæmorrhage in typhus since.

DR. HAYDEN.—The Society is greatly indebted to Dr. Kennedy for his practical paper, but it seems to me that we must be very cautious in accepting his conclusions as to hæmorrhage in fever. No doubt many examples can be had in which a fatal issue did not follow the occurrence of hæmorrhage in fever. This is frequently the case in typhus, and it is occasionally the case in typhoid, fever; but I think those cases constitute by far the smaller number, and, notwithstanding Dr. Kennedy's conclusions, I regard the occurrence of hæmorrhage in fever, even in typhus fever, as a very formidable symptom. I have seen cases of hæmorrhage in typhoid fever in which no ulceration occurred. One of those cases Dr. Kennedy alluded to, where death followed from a rapid flux from the large intestine, and in which there was no source of the hæmorrhage, save and except the extraordinary hyperæmia of the large intestine. Here it would have been very desirable to have diagnosed the absence of ulceration and the source of the hæmorrhage. That would have been a case in which turpentine might have been used with good effect. It would have been important if we had diagnosed the source of the hæmorrhage, because we might have been able to act locally by the injection of ice-water into the lower bowel; but, unfortunately, in many cases where there was no evidence whatever of existing ulceration, *post-mortem* examination has proved the existence of extensive and advanced ulceration, so that the absence of symptoms that might lead one to conclude that ulceration did not exist, would not warrant us in assuming that there was no ulceration; and, on the other hand, the occurrence of symptoms that would lead to the conclusion that ulceration did exist, might be fallacious. I would rely more upon a high range of temperature and a high pulse-rate, conjoined with tympany and localised abdominal tenderness, as evidence of ulceration, than upon any other symptoms. In the case I brought forward at the Pathological Society last session, the range of temperature was never above 103°, and the pulse was not remarkably quick. The hæmorrhage was so copious in that case that it saturated the bed, and streamed down to the floor of the ward, and the poor man on the second accession of it died very rapidly, the interval between the first and second attack being about four hours. I have risen chiefly because I should fear very much to encourage the impression that Dr. Kennedy's paper is calculated, although, perhaps, not meant, to produce—namely, that hæmorrhage in fever is not to be regarded as a formidable symptom. I think it is a formidable symptom, and that the cases in which it is not a fatal symptom are the smaller number. I can recall two or three examples in typhoid fever in which it

was not fatal. Dr. Kennedy says the non-fatal cases were generally persons under twenty years of age. That is also my impression. Two cases of hæmorrhage in typhoid which came under my observation, and which were not fatal, were females under twenty. The hæmorrhage was copious and recurrent, and yet the women recovered.

The CHAIRMAN.—As regards my own experience of hæmorrhage in typhus fever, my recollection is that the majority of the cases recovered, but I quite agree with Dr. Hayden in saying that I have always looked upon it as a most formidable symptom, and in many of those cases where the patients recovered their recovery was contrary to all reasonable expectation, they had been reduced to a state of such extreme exhaustion. I have always looked upon hæmorrhage in these cases as a most alarming symptom. I quite agree with Dr. Kennedy in most of the remarks he has made, particularly as to the mixed cases in which the hæmorrhage occurs. I saw no later than yesterday a case of typhoid fever—a case, at least, of fever in which enteric symptoms prevailed, although there was no typhoid rash, not a single spot, but from the symptoms, the temperature, &c., I looked upon it as a case of enteric fever. The serious complication at first was very intense bronchitis of the minute bronchial tubes, with general pulmonary congestion, simulating rapid tuberculosis, but some circumstances in the case made me yesterday predicate that it was a case in which there was likely to be hæmorrhage. The symptoms were the elevation of temperature and increased pallor, and, unfortunately, my prognosis was verified last night. There was alarming hæmorrhage from the bowels, which recurred two or three times this morning, and, although it relieved the pulmonic congestion, it left the patient in a very alarming state. It was a well-marked example of hæmorrhage from the bowels occurring in the second or third week in a fever not marked by any eruption of any kind whatever, but in which the symptoms of enteric fever were more prominent than those of any other form. I should have wished that Dr. Kennedy had gone into the question of the treatment of such cases. This time twelve months I saw a very alarming case of typhoid fever in a young person, twenty-two years of age, in which there were three accessions of hæmorrhage, at intervals of seven or eight days. The treatment adopted was the employment of stimulants sufficient to keep life going, and the sub-cutaneous injection of ergotine, and the effect of its use was marvellous. In many of these cases where turpentine is desirable the patients will not bear it; it produces irritability of the stomach, and cannot be employed. In the present case ten grains of ergot given every two hours was successful in, for the present at all events, checking the hæmorrhage.

DR. KENNEDY, in reply, said he had intended to speak of the dicrotous

pulse, which many thought went hand-in-hand with the hæmorrhage which he had brought before their notice. No doubt, the two were often met together, but he had notes of cases where it was not so. He was glad to find that Dr. Darby had confirmed the fact, that typhus cases might be complicated with hæmorrhage from the bowels, and also might recover. In reference to the remark of Dr. Hayden, he did not for a moment mean to state that these cases of intestinal bleeding were not serious—far from it; but whilst all were, in one sense, grave, it was not to be forgotten that the ultimate result was more favourable than could, *à priori*, have been anticipated, being in his own experience, and as already stated in the paper, one death in every four cases, thus confirming the opinions of Graves, Trousseau, and others.



# PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

---

## THIRTY-SIXTH ANNUAL SESSION.

---

*Saturday, 9th May, 1874.*

EVORY KENNEDY, M.D., President, in the Chair.

*On Puerperal Convulsions.* By THOMAS MORE MADDEN, M.D., M.R.C.S.E.; lately Examiner in Midwifery and the Diseases of Women, in the Queen's University in Ireland; Physician to St. Joseph's Hospital for Sick Children; Ex-Assistant Physician, Rotundo Lying-in Hospital; Corresponding Fellow of the Obstetrical Society of Edinburgh, and of the Gynæcological Society of Boston, &c., &c.

THERE are few obstetric questions of greater interest than the etiology, prevention, and treatment of puerperal convulsions. Some advance has been recently made in the prophylaxis and management of this disease, but its causes remain *sub judice*, and little can yet be added with certainty to what Hippocrates wrote:—"Σπάσμος ἢ ὕπο πληρώσιος ἢ κενώσιος:" though from his time to the present almost innumerable theories had been applied to this subject.

I shall now lay before the Society the history of a few of the cases of puerperal convulsions that have come under my notice in hospital and private practice, together with some general observations on the disease, and a reference to the principal views that have prevailed at different times as to their nature and treatment. Several of the opinions thus referred to, though quoted from writers now seldom consulted, are nevertheless of interest. For, in investigating subjects like the present, which have long engaged and baffled inquiry, it is surely not unworthy of a scientific society, however practical and devoted to progress, occasionally to look back to what has been done by those who have been pioneers in those obscure paths of inquiry which we would ourselves explore.

Convulsions are, with the exception of rupture and inversion of the uterus, the most dangerous as well as the least frequent of the complications of labour. The relative frequency as well as the danger of this disease is shown by the following table, compiled from Reports of the Masters of the Dublin Lying-in Hospital.

TABLE.

Date	Master	Deliveries	Convulsions	Primiparae	Twin Births	Result to Mother		Sex of Children		Living	Still-born	Mode of Delivery				
						Recovered	Died	Male	Female			Natural	Forceps	Version	Vectis	Craniotomy
1787-1793	Dr. Clarke	10,387	19	16	—	—	—	—	—	—	—	—	—	—	—	—
"	Dr. Collins	16,654	30	29	2	25	5	20	10	14	18	15	6	—	—	8
1842-1845	Dr. C. Johnson (Reported by Drs. Hardy and M'Clintock)	6,702	13	10	—	10	3	6	7	7	6	6	2	—	1	4
1847-1854	Dr. Shekleton (Reported by Drs. Johnston and Sinclair)	13,748	63	49	5	50	13	36	33	45	25	36	24	2	—	7
1868-1869	Dr. Johnston	1,159	4	—	—	—	—	—	—	—	—	—	—	—	—	—
1869-1870	Do.	1,087	5	2	1	4	1	4	2	4	2	—	3	1	—	1
1872-1873	Do.	1,191	4	3	—	—	4	—	—	—	—	—	4	—	—	—
	Total	50,928	138	109	9	89	36	66	52	70	51	57	39	3	1	20

I. *Etiology of this disease.*—The theory that puerperal convulsions are reflex actions excited by cerebro-spinal or medullary irritation, of uterine origin, and transmitted through the ganglionic cells in which the reflex nerves terminate, has been formulated by several recent writers, but may be traced back to Laurence Joubert, who, during the middle of the sixteenth century, was Professor of Medicine in the University of Montpellier. In his essay on convulsions, this once well-known author not only controverted the Hippocratic aphorism already quoted, but, moreover, asserted that the cause of convulsions is irritation, and that only by the removal of the source of this irritation can the paroxysms be arrested.<sup>a</sup>

The analogy between the abnormal nervous action thus excited, and the effects of an electrical discharge has been remarked by obstetricians as well as physiologists from the time of William Hunter. The proximate cause of this disease must primarily effect the central excito-motor portion of the nervous system. Reflex actions are now generally referred to the medulla oblongata, and the researches of Dr. Brown-Séquard support the opinions of Van der Kolk, Kussmaul and Tenner, as well as those of Dr. Marshall Hall and other older writers, in assigning the upper part of the spinal cord, the medulla oblongata, and pons varolii, where the roots of the first motor nerves have their origin, as the probable starting point of the convulsive action in these cases. In proof of the influence of physical impressions on the medulla oblongata in producing convulsive action, I may refer to two cases of acephalous fœtuses which came under my observation some years ago in the Lying-in Hospital. One lived for twenty minutes, and the other for an hour and a half after birth, and in both it was remarked that the slightest pressure on the bulbous expansion of the medulla oblongata, which supplied the place of the brain, produced violent general convulsions.

The older British obstetricians, with a few exceptions, held that puerperal convulsions were generally occasioned by determination of blood to the head, and should be treated by blood-letting. This was the teaching of Scott,<sup>b</sup> of the Hamiltons,<sup>c</sup> Smellie,<sup>d</sup> Denman,<sup>e</sup> Bland,<sup>f</sup> Foster,<sup>g</sup> William

<sup>a</sup> Joubert, de Convulsionis Essentia et Causis, Op. An. p. 219, Ed. Antwerp. 1500.

<sup>b</sup> Lectures on Midwifery. By Robert Scott, M.D. 1775. P. 113.

<sup>c</sup> A Treatise on Midwifery. By Alexander Hamilton, M.D. P. 199. Edinburgh. Dr. Hamilton's Lectures on Midwifery, in the University of Edinburgh, 1815-16; reported by Dr. M'Keever. MS. P. 65.

<sup>d</sup> A Collection of Cases and Observations in Midwifery. By William Smellie, M.D. Vol. ii., Collect. xviii., No. 5. London, 1779.

<sup>e</sup> Introduction to Midwifery. By Thomas Denman, M.D. P. 430. Ed. Edinburgh, 1781.

<sup>f</sup> On Human and Comparative Parturition. By James Bland, M.D. P. 138. London, 1794.

<sup>g</sup> Principles of Midwifery. By Edward Foster, M.D. P. 118. London, 1781.

Hunter, and other eminent men-midwives of the eighteenth century. The same theory being propounded by Davis,<sup>a</sup> Ryan,<sup>b</sup> Blundell,<sup>c</sup> Burns,<sup>d</sup> Maunsell,<sup>e</sup> and others, was, down to a comparatively recent period, generally accepted as a sufficient explanation of the causes of convulsions. In America, too, according to a very able writer, "we find no other idea but congestion of the head is entertained as the cause of puerperal eclampsia."<sup>f</sup> And this theory is reiterated in the principal manuals used by students in that country.<sup>g</sup> I might easily add a much longer list of Continental as well as of British and American authorities to the same effect. But fully enough have been adduced to prove the widespread acceptance of this opinion.

Pregnancy may, to some extent, be regarded as a predisposing cause of cerebro-spinal congestion. The blood at this time is not only increased in quantity, but also contains more fibrine than usual. As gestation advances the enlargement of the uterus increases the tension of the cerebral vessels, which attains its maximum during the violent efforts of parturition when puerperal convulsions most frequently commence.

It has been argued by Dr. Inglis and others, that the circumstance of eclampsia commonly beginning at night is a proof that the disease is connected with congestion of the brain. This fact does not, however, appear to me to support the opinion thus founded upon it. For, it is now generally held that during sleep the brain is in a comparatively bloodless condition; and the blood in the encephalic vessels is not only diminished in quantity, but moves with diminished rapidity.<sup>h</sup>

Convulsions are not confined to plethoric patients; and it is unquestionable that anæmia, whether resulting from the sudden loss of blood

<sup>a</sup> Principles and Practice of Obstetric Medicine. By D. D. Davis, M.D. Vol. ii. P. 1027. London, 1836.

<sup>b</sup> A Manual of Midwifery. By Michael Ryan, M.D. P. 519. Third Ed. London, 1831.

<sup>c</sup> Principles and Practice of Obstetric Medicine. By James Blundell, M.D. P. 424. London, 1831.

<sup>d</sup> Principles of Midwifery. By John Burns, M.D. P. 519. Tenth Ed. London, 1843.

<sup>e</sup> The Dublin Practice of Midwifery. By Henry Maunsell, M.D. Edited by Thomas More Madden, M.D. P. 194. Sixth Ed. London, 1871.

<sup>f</sup> Principles and Practice of Obstetric Medicine and Surgery. By Francis Ramsbotham, M.D. P. 449. First Ed. London, 1844.

<sup>g</sup> On Puerperal Eclampsia. (Review) American Journal of Medical Science, April, 1869. P. 437. Conspectus of Medical Sciences. Edited by H. Hartshorne, M.D. P. 997. Philadelphia, 1869.

<sup>h</sup> Dr. Inglis—Facts and Cases in Obstetric Medicine, p. 7: London, 1836. Mr. Durham—On the Physiology of Sleep, Guy's Hospital Reports, p. 24: London, 1860. T. More Madden—On Dreaming considered in Relation to the Study of Insanity, read before Med. Soc. Coll. Physicians, Dublin; Medical Press and Circular, 1869.



by hæmorrhage, or from the gradual deterioration of the vital fluid by disease, is conducive to eclampsia.

Nor is pregnancy, even when apparently accompanied by plethora, actually so in most cases. On the contrary, the blood, though increased in quantity, is then more generally impoverished, containing fewer corpuscles, less albumen, and a larger proportion of water, by which the circulation is more and more embarrassed as the uterus enlarges. This vascular tension occasionally results in serous effusions into the areolar tissue or serous cavities, and the discharge of albuminous urine, by which a considerable drain of the nutritive elements of the blood is produced.

The connexion between general dropsy and convulsions was pointed out by Dr. Hamilton, of Edinburgh, in the year 1800. Dr. Blackwell next showed that albuminuria was present in these cases; and about the year 1835 it was discovered by Dr. Bright, that this was connected with granular degeneration of the kidneys. The field of investigation thus opened was further explored, with a special reference to the pathology of puerperal eclampsia, by Dr. Lever, M. Robin, M. Becquerell, M. Frerichs, Professor Braunn, and others, by whom it was proved that the convulsions of pregnancy are frequently associated with dropsy, marked by albuminuria, and attended by the diminished excretion of urea and uric acid, and the consequent retention of these compounds in the system.

In cases of this kind the urine is not invariably albuminous. I have examined this secretion in six instances of convulsions during labour, and in only four of them was albumen discovered. On the other hand, I have found albuminuria in pregnant women who had no subsequent attack of eclampsia.

In two cases of stenic convulsions I had an opportunity of testing the blood for excess of urea, but was unable to detect any appreciable trace of this salt on a microscopic examination of the evaporated serum, treated in the ordinary manner with nitric acid. Either urea or carbonate of ammonia, resulting from its decomposition, are frequently present to an abnormal extent in such cases; though both these salts may be injected into the blood of a healthy animal without producing convulsions.

That convulsive action may be occasioned by blood-poisoning is well known in other diseases in which—as, for instance, in small-pox—severe jaundice, morbus Addisonii, Bright's disease, and during recovery from scarlatina, convulsions occasionally result from this cause. And during pregnancy a similar effect is not improbably produced by the pressure of the gravid uterus on the renal emulgent veins interfering with the functions of the kidneys, as well as acting as a cause of cerebro-spinal congestion.

In considering the causes of puerperal eclampsia, we must bear in mind the various conditions under which ordinary epileptiform convulsions occur. Many agree with Trousseau<sup>a</sup> in regarding these as identical; and certainly Cullen's definition of epilepsy applies to the disease we are now considering—*musculorum convulsio cum sopore*.<sup>b</sup> Dr. Radcliffe<sup>c</sup> has shown that epileptiform convulsions occur in connexion with almost every variety of cerebral disease as well as in the moribund state, and as a consequence of reflex irritation.

That great hæmorrhage is productive of convulsions is known to every accoucheur who has had to witness a case of fatal *post-partum* flooding; therefore it is unnecessary for obstetricians, at least, to dwell on the elaborate experiments by which this fact has been established. The convulsions produced by hæmorrhage, like those arising from the circulation of impure or vitiated blood, result from the interruption of that regular and sufficient supply of healthy blood to the nervous centres, which is essential to their normal action, and the sudden withdrawal of which by hæmorrhage, or its gradual deterioration by disease, are alike probable causes of these irregular manifestations of disordered nerve force.

Putting aside the distinction between the proximate and the predisposing causes of this disease, which I believe are so inextricably interwoven that it would be impossible to consider them separately, from the foregoing abstract of the different opinions which have prevailed on this subject, read by the light of my own clinical observations, I would venture to draw the conclusion, that in the causation of puerperal convulsions a variety of circumstances have a share, and must be taken into equal account. In the first place the disease is obviously connected not only with the state of the uterus itself, and with that of the adjoining organs during gestation, but still more so with the remarkable condition of nervous susceptibility peculiar to pregnancy. In the cases under consideration, the cerebro-spinal nervous centres are usually more or less congested, even when the patient's general condition is anæmic, and are irritated by the circulation of vitiated blood containing some non-eliminated *materies morbi* through their vessels, producing a direct toxic effect on the excito-motor nerve substance of the brain and medulla oblongata, and stimulating the hyperæsthetic condition just referred to till the latent excitability becomes so intense that it needs only the addition of uterine irritation, such as the first pain of labour, to cause the pent-up nerve force to burst into uncontrollable action, and produce the violent reflex muscular spasms that constitute puerperal convulsions.

The season, the age of the patient, her temperament, and the fact of

<sup>a</sup> Trousseau—Clinical Medicine. Vol. i., p. 32.

<sup>b</sup> Cullen—Synopsis Nosologiæ Methodicæ. Edit. 3. 1780.

<sup>c</sup> Radcliffe, on Epilepsy and Convulsive Affections. 2nd Edition, p. 262.

its being her first pregnancy or not, have also a considerable influence in the causation of this disease.

It is a remarkable fact, that puerperal convulsions generally attack a number of individuals almost simultaneously. The disease is by no means a common one; and yet, of the few instances of it which I have seen during the last six years, no less than three occurred within one fortnight. Madame Lachapelle and Dr. Ramsbotham both make a similar observation. The former says—"When one of our women is taken with convulsions, we rarely fail to have soon afterwards others in the same state." The latter observes—"I have repeatedly remarked amongst the numerous patients of the Royal Maternity Charity, as well as amongst others to which I have been accidentally called, that several cases have occurred soon after each other." And it certainly seems not improbable, as was long since conjectured by Smellie<sup>b</sup> and Denman,<sup>c</sup> whose opinions have been confirmed by M. Andral,<sup>d</sup> Dr. Inglis,<sup>e</sup> as well as by Dr. Hall Davis<sup>f</sup> and other recent writers, that the explanation of this circumstance will be found in the occurrence of some peculiar electrical condition of the atmosphere at the time these manifestations of disordered nervous action are most rife.

II. *Presentation*.—In almost every instance of puerperal eclampsia that I have met with, the presentation was natural; and the experience of most other practitioners is similar to my own on this point.

III. *Plural Births* are most frequently complicated with convulsions.

IV. *Influence of Convulsions on Parturition*.—Whenever eclampsia occurs towards the end of pregnancy, labour is produced by the disease. If it commences after labour has set in, the delivery is generally rather accelerated by their complication.

V. *Effect of Mental Impressions in Causing Convulsions*.—This has been remarked by all obstetricians since the time of Denman, by whom it was most ably and fully discussed. Anxiety of mind, depression of spirits from reverse of circumstances, sudden shocks, are conducive of eclampsia; and some one of these, or still more commonly the combination of shame, anxiety, and sorrow in unmarried women, were clearly predisposing causes of this disease in almost every case that I have seen.

VI. *Primiparae are most liable to Convulsions*.—Thus, in the cases of eclampsia which have come under my observation, five occurred in cases

<sup>a</sup> Dr. Ramsbotham—*Obstetric Medicine and Surgery*, p. 451. London, 1844.

<sup>b</sup> Smellie—*Midwifery*. Vol. ii., p. 285. London, 1779. Vol. iii., p. 161. London, 1789.

<sup>c</sup> Denman—*Introduction to Midwifery*.

<sup>d</sup> M. Andral—*Clinique Médicale*. Translated by D. Spillan, M.D. P. 77. Lond., 1836.

<sup>e</sup> Dr. Inglis.

<sup>f</sup> Dr. Hall Davis, on Puerperal Convulsions. London Obstetrical Society. Vol. xi., p. 274. London, 1870.

of first labour, and three in subsequent confinements. The same remark has been made by nearly every other writer on the subject, and is borne out by the Table I have constructed from the reports of the Rotunda Hospital, by which it appears that of 138 patients attacked by convulsions, 109 were primiparæ, and only 29 were multiparæ.

VII. *The Classification of Puerperal Convulsions* into hysterical, epileptic, and apoplectic, may, I think, be entirely disregarded. This disease differs essentially in its nature and causes from either epilepsy or apoplexy, being a convulsive affection *sui generis* peculiar to women who are either pregnant or soon after parturition.

The hysterical form of puerperal convulsions being merely ordinary hysteria occurring in the early months of gestation, though possibly excited by reflex uterine irritation, requires no peculiar treatment nor further notice. Epileptiform and apoplectiform convulsions are identical in their origin and nature, approaching each other in widely varying degrees in different cases, and influenced in their symptoms by the severity of the attack and the constitutional state of the patient, rather than by any essential difference in the nature of the disease.

VIII. *Premonitory Symptoms*.—In the majority of cases puerperal convulsions are preceded by œdema of the upper extremities, face, and eye-lids, pain in the lumbar region, and albuminaria. For several days before the attack the patient generally complains of malaise, followed by head-ache, giddiness, confusion of thought, or peculiar irritability of temper, similar to that which is occasioned by the circulation of lithic acid in the blood, and which precedes an attack of gout.

IX. *Symptoms of Asthenic or Epileptiform Puerperal Convulsions*.—The phenomena of the complete seizure are somewhat similar to those of an ordinary epileptic fit. Commencing with twitching of the muscles of the eye-lids and eye-balls, the convulsions soon increase in violence, extending to every part of the body (though in every case that I have seen they were more marked on one side than on the other), and recur at irregular intervals, in clonic spasms of varying duration and intensity. In anæmic patients throughout the attack the face may be cool and pale, the eye glistening, and the pupils contracted. In the majority of cases the patient's state during the commencement of the attack is that of vascular depression, rather than of vascular excitement; the extremities being cold, the countenance pallid, and the pulse, though quick, weak and compressible. But generally as the convulsions recur more frequently, the impeded respiration and consequent non-aëration of the blood induces symptoms of venous congestion; the face becomes dusky and livid, the lips and ala nasi turgid, the breathing hissing or stertorous, the pulse full and labouring; and thus the disease passes from the first into the second stage, or from the so-called epileptiform into the so-called apoplectiform convulsions.



X. *Stenic or Apoplectiform Convulsions*.—In plethoric women the disease generally presents, *ab initio*, the apoplectiform character, and may commence by a sudden violent convulsion, after which the patient falls into a comatose state, in which she lies, as well described, “like a person dead drunk,” the convulsions meanwhile recurring at irregular intervals. Her face is congested, the carotids and temporal arteries throb visibly, the respiration becomes stertorous, the pulse slow and full, the limbs placid, and no reflex action responds to any external stimulation. After remaining for an uncertain time in this condition, midway between life and death, under favourable circumstances the convulsions may cease, and the patient at last slowly regains consciousness, and awakes once more to renewed vitality, though her mental powers will probably remain clouded for some days. But, on the other hand, the coma may become more profound, the pulse slower and more labouring, the respiration more embarrassed, the face more pallid, the extremities colder, and the skin covered with a clammy moisture, until at length “the last sad scene of all” is closed by a violent and final convulsion.

These convulsions may occur at any time of pregnancy, during labour, and within the puerperal period. Most commonly they begin with the dilatation of the os.

XI. *Treatment*.—The treatment of puerperal convulsions must be considered in reference to the state of the patient in each instance.

In all cases prevention is better than cure, and hence the importance of an early recognition of the premonitory symptoms, as by timely prophylactic measures we may sometimes succeed in warding off impending convulsions.

In this prophylactic treatment our objects are—first, to relieve the kidneys; secondly, to assist the efforts of nature to purify the blood; and, thirdly, to soothe the nervous irritability peculiar to these cases. The first object may be attempted by cupping and fomentations over the loins, the free use of diluents, and the cautious administration of mild diuretics, and especially by colchicum, in small and guarded doses. The second intention may be fulfilled by saline aperients as well as by diaphoretics, if the skin be harsh and dry, and the third by sedatives, especially bromide of potash and belladonna.

The therapeutic indications in cases of puerperal eclampsia are—first, to arrest the convulsive action; and, secondly, to remove the cause of its recurrence.

During the convulsions the ordinary precautions, such as loosening the patient's clothing, and preventing her from biting her tongue, by inserting any suitable substance between the teeth, or from injuring her person in any way by proper restraint, should, in the first instance, be attended to.

One of the most effectual means of shortening the paroxysms is cold

affusion in a small stream from a moderate height on the head and face. This remedy is of considerable antiquity, being recommended by Valescus, of Tarenta, in a work<sup>a</sup> originally published in the year 1482. It was re-introduced into practice on the authority of Denman, who derived great benefit in such a case by merely sprinkling his patient's face with cold water during the paroxysms—a very different practice, I may observe, from the copious cold affusions now recommended. In the asthenic form of eclampsia this remedy should be used cautiously. It should not be employed except during the convulsions, nor persevered in so long as to depress the circulation unduly.

In all cases the *primæ viæ* should be unloaded, as soon as the convulsions commence, by a bolus of calomel and jalap, or by a drop of croton oil placed on the tongue. Enemata of assafœtida and turpentine, suspended in thin gruel, may also be resorted to, and repeated if necessary.

The head should be shaved if possible, and the back of the scalp freely painted over with liquor epispasticus, whilst, at the same time, a bladder loosely filled with ice may be laid over the front of the head. The feet and calves of the legs should be enveloped in mustard poultices, until a decided rubefacient effect is produced.

In cases of stenic puerperal convulsions, *venesection* is, notwithstanding the disusage into which blood-letting has fallen in all other diseases, still the only remedy of undoubted efficacy in subduing the convulsive action. If the patient be plethoric, and her pupils be contracted, showing cerebral congestion, we may, as a rule, bleed. If, on the contrary, the pupils are dilated, the condition of the brain may be considered as anæmic, and blood-letting would probably be out of the question. This rule is liable to many well-known causes of exception, as the state of the pupil may normally vary widely in different individuals, as well as be affected by various toxic agents.

The amount of blood that may be taken from a plethoric woman, suffering from eclampsia, should be measured by the patient's condition and the effect produced, rather than by the quantity abstracted. In one case I took nearly forty ounces of blood, and within a few hours twelve ounces more, but without any benefit. Generally, however, a very much smaller bleeding will suffice, and, as a rule, not more than from eight to twelve ounces of blood should be taken.

*Chloroform* is still regarded by some authorities as the remedy *par excellence* for puerperal convulsions: and though, according to my experience, this is an exaggerated estimate of the value of this anæsthetic, its inhalation is of unquestionable use in many cases. In hysterical convulsions, if sprinkling the face with cold water does not

<sup>a</sup> Valescus de Tarenta, *Philon. Pharmaceut. et Chirurg.* Lib. i., c. 27, p. 92. Franca, 1599.

suffice, a few whiffs of chloroform will generally cut short the attack. In true puerperal convulsions, however, in which I have used chloroform pretty extensively in the manner originally suggested by the late Sir James Simpson, and have kept patients under its influence for several hours at a time, it requires to be used with great caution, its exhibition being obviously contra-indicated where either the circulation is depressed, or where there is any tendency to apoplectiform symptoms. But in suitable cases I have found chloroform most serviceable in subduing the convulsions and prolonging the intervals between them. If it be inhaled only during the paroxysm, chloroform appeared to have no effect in shortening the attack; but if exhibited before its expected return, it often prevents its recurrence for hours together, and gains time, during which the labour may be completed, and the patient placed in comparative safety.

*Chloral* was suggested by myself in a paper published four years ago, and has since been employed with varying success by other practitioners in England and America.

*Opium*, though recommended upon high authority,<sup>a</sup> is, in my opinion, clearly contra-indicated in all cases of eclampsia during labour in which there is any tendency to apoplectiform symptoms.

*The Tincture of Veratrum Viride* has been used as a substitute for blood-letting in cases of puerperal convulsions by Dr. Fearn, of Brooklyn. Dr. Fearn exhibited this remedy in very large doses in ten cases of this kind—"there being," he says, "no danger from the medicine as long as the convulsions continue."<sup>b</sup> I should, myself, prefer some safer plan of treatment than these heroic doses of so powerful a drug.

*Belladonna* was originally introduced into practice in these cases by M. Claussier fifty years ago,<sup>c</sup> and has again been recommended by recent writers. My own experience in those cases in which I have seen it tried, would not lead me to attach any value to this drug in the treatment of eclampsia during labour. But in convulsions occurring before and after parturition, I have found small doses of belladonna most beneficial in calming the nervous susceptibility so intimately connected with convulsive action.

In every case of convulsions during labour our primary object should be to deliver the patient as speedily as is consistent with her safety and that of the child. This rule of practice was long since pointed out by Mauriceau—"La convulsion est un autre accident qui fait souvent perir

<sup>a</sup> Manning on Female Diseases, p. 357: London, 1775. Romberg, a Manual of the Nervous Diseases of Man, Sydenham Society, Translation, Vol. II., p. 190: London, 1853. Schwartz, Ueber Eclampsia der Kreissenden, p. 54: Riga, 1851.

<sup>b</sup> Fearn, American Journal of Obstetrics, May, 1871, p. 28.

<sup>c</sup> Claussier, Considerations sur les Convulsions qui attaquent les Femmes Encientes: Paris, 1823.

la mere et l'enfant, si la femme n'est très promptement secourue par l'accouchement qui est le meilleur remède qu'on puisse apporter à l'une et à l'autre." <sup>a</sup>

The convulsions do not always cease when delivery is effected, or may even commence after it. Still these cases afford no argument against the general principle that, puerperal convulsions being obviously connected with the state of the gravid uterus, the sooner this condition is terminated the sooner will the convulsions cease. The manner of accomplishing this purpose must depend on the stage and character of the labour in each case. But if the symptoms be at all urgent, the former consideration may be in a great measure disregarded, and we should not then hesitate to deliver our patient by either version or the long forceps as soon as the os uteri can be opened sufficiently to enable us to do so. In these cases only, despite Dr. Blundell's excellent aphorism, "meddlesome midwifery," is not necessarily "bad midwifery."

With regard to the manner of effecting this, as a rule the dilatation of the os goes on during the convulsions, and by keeping our patient under chloroform we may generally attend the natural occurrence of the second stage of labour before being obliged to deliver. But in some cases, as I very recently had an instance, the os, after expanding to a certain extent, becomes rigid and undilatable, the convulsions meanwhile recurring with increasing violence. In such cases the perforator and crochet were formerly freely resorted to. Thus, in no less than eight of Dr. Collins' thirty cases of convulsions, delivery was effected in this way. I cannot regard embryotomic or child-destroying operations as justifiable, even in these cases, for we now have it in our power to effect delivery without resorting to them, by dilating the os uteri with Dr. Barnes' dilators, or, where these fail, by incising the contracted circular fibres of the os with a guarded bistoury, as originally suggested by M. Dubosc of Toulouse, in 1781, so as to allow a living child to be delivered. Such an operation should, however, be only regarded as the *ultima spes*, and confined to those rare cases in which the delivery of a living child from a living mother cannot be effected by less hazardous means.

CASE I.<sup>b</sup>—(Reported by Dr. F. Butler, then resident in the hospital). Mary Corby, aged eighteen, first pregnancy; duration of labour seventeen hours, complicated with apoplectic convulsions and plurality of children. First child, head presentation, delivered (dead) with forceps. Second child, footling presentation, lived only two or three minutes.

*History and Treatment.*—At 2 o'clock, p.m., on October 21st, when first

<sup>a</sup> Traite des Maladies des Femmes Grosses, par François Mauriceau, 7th Edition, Tome Premiere, p. 335: Paris, 1740.

<sup>b</sup> I am indebted for the reports of several of these cases to the notes of gentlemen who were at the time resident in the Rotunda Hospital.



seen patient was suffering from a paroxysm of apoplectic convulsions; cold water and vinegar were applied to the vertex and nape of the neck; after fifteen minutes' application without any good result, Dr. Madden was sent for and advised the cold douche, which was tried and continued for thirty minutes, but without relieving the paroxysm; it, however, reduced the frequency of the pulse from 145 to 80 beats per minute. On examination per vaginam the os was found to be dilated to the size of a shilling.

Dr. Madden administered calomel gr. v., and proposed depletion by bleeding from the arm, but as the paroxysms were almost continuous, he was unable to do so until chloroform was administered, which immediately checked the fit. She was now bled from the right arm and took ℥xij. of blood; previous to the bleeding her pulse had risen to 140, but after it fell to 72 per minute. The patient was then (4 45) removed to the hospital, being still under the influence of chloroform.

When the patient came into hospital the hair was closely cut from the back of her head and vesicating collodion applied.

At 7, Dr. Denham visited her and ordered sinapisms to the calves of her legs and the soles of her feet, and an enema of turpentine, castor-oil, and assafoetida to be administered, which only partially relieved the rectum. At 10 30 the membranes ruptured, the os being about the size of a five-shilling piece, and the head presenting.

At 10 40, the patient's pulse being 154, full and bounding, and her respiration stertorous, Dr. Madden again bled from the arm and took ℥xxxviii. of blood; a bladder of ice was applied to her head; the pulse did not diminish in frequency, but became small and compressible, nor was the florid colour of her lips at all altered.

At 11 30 an enema, the same as before, was administered, and hot stupes applied to her feet every fifteen minutes for an hour.

At 12 30 we administered croton oil ℥ij.

At 12 50, on examination, the os was found fully dilated.

At 12 55, Dr. Madden applied his forceps and delivered the first child (dead). The Master now examined and found there was a second child, footling presentation, which was delivered, but only lived for three minutes.

Seven minutes after the birth of the second child both placentæ came away. After delivery the patient appeared to be sinking, and sinapisms were applied to the calves of her legs and over her heart, and an enema of beef-tea and brandy administered and repeated every hour until death ensued, at 2 30 p.m., on the 22nd.

Leave having been obtained to examine her head, a *post-mortem* examination was made at 8 p.m.; the pia mater was congested, but there were no clots found, nor was there any serous effusion.

Subjoined is a list of the paroxysms:—

The first paroxysm was felt by the patient at 9 a.m. on the 21st, and she had five fits before she was seen at 2 p.m.

Duration of Fit. minutes	Interval. minutes	Characters	Duration of Fit. minutes	Interval. minutes	Characters
2	18	General.	2	2	Confined to head, body and upper extremities
2½	18	Do.			General.
2	2	Do.	2	57	Do.
3	23	Do.	2½	18	Do.
2	3	Do.	1	12	Do.
2	5	Do.	2	62	Do.
2	10	Do.	3	20	Do.
3	5	Do.	2	17	Do.
2	25	Do.	2½	30	Do.
2	10	Confined to body, head and right arm.	2	30	Do.
1½	3	Do.	1½	20	Do.
2	30	Do.	2	15	Do.
4	1	Do.	2	15	Do.
1	5	Do. and left arm.	2½	15	Do.
2	10	Do.	2	15	Do.
1	48	Do.	2	20	Do.
2	20	Confined to head, body and upper extremities	1½	20	Do.
3	27	Do.	2	20	Do.
2	22	Do.	3	15	Do.
			2	15	Do.

During each paroxysm chloroform was administered until the fit terminated; at first this treatment was attended with marked success, but afterwards did not prove so efficacious.

CASE II.—Rosanna Mortimer, aged twenty-eight, first pregnancy, was admitted into hospital, October 8th, 1869, being eight months pregnant. She had five attacks of epileptiform convulsions during that day, commencing at 5 p.m. in the afternoon. When admitted she was in a semi-comatose condition, cold affusion was immediately resorted to with sinapisms to the legs and feet, and turpentine and assafœtida enemata. She soon became conscious, had no return of the fits, but still complained of head-ache and confusion of thought. One grain of extract of belladonna was ordered every fourth hour. On the 10th she was delivered of a healthy living male child, weighing 5 lbs., after a natural labour of nine hours, and made an excellent recovery.

CASE III. (Reported by Mr. Roche, then resident).—M. R., aged twenty-two; first pregnancy; married ten months; labour commenced at 1 p.m., November 15th, and terminated at 1 a.m., on the following morning. The child was a male, and was born alive, and the placenta was expelled immediately. Shortly afterwards she had in rapid succession three attacks of convulsions. At 2 a.m. Mr. Roche saw her, and found her unconscious, and suffering from hæmorrhage. On examination, a small piece of membrane was found in the os, and being removed, the hæmorrhage ceased. At 2 30 a.m., she had a fit of an apoplectic

kind, and up to 6 o'clock a.m., when she was removed to the hospital, she had had seven of these seizures, with an average interval of half an hour between them. During the fits, the eye-balls were turned up, the pupils widely dilated, tongue protruded and bitten, frothing at the mouth, lips blown outwards with the violent expirations and a peculiar jerking lateral movement of the lower jaw. The head, neck, and upper extremities, were the parts principally affected during the fits.

Eight grains of calomel and a drop of croton oil were now administered and followed by a foetid enema. The head was shaved, ice was applied, hot mustard stupes to the calves of the legs and soles of feet, and blister to the nape of the neck.

From 6 a.m. to 2 15 p.m., she had nine fits, with an interval of three-quarters of an hour between them. They then became more frequent, till a quarter past eleven o'clock p.m., during which time—*i.e.*, from 2 15 till 11 15 p.m.—she had eighteen fits. There was then an interval of one hour and twenty-five minutes, followed by a fit, and at 1 15 a.m., November 17th, the last fit occurred. The total number of convulsive seizures was thirty-six.

At 3 50 p.m., November 16th, her pulse being 116, hard, full and bounding, and the fits recurring very frequently; twenty ounces of blood were taken from the arm; the pulse now rose to 154, but became soft and compressible, and the frequency of the epileptiform seizures diminished. Midnight on the 16th, the following was directed:—

R Extract of belladonna,	-	-	gr. ii.
Chloric ether,	-	-	℥ xl.
Aromatic spirits of ammonia,	-	-	℥ xl.
Beef-tea,	-	-	℥ iv.

℥i. to be injected every 3rd hour, ice bag to head, and the evaporating lotion. Under this treatment the symptoms of eclampsia rapidly subsided, and on their cessation the following day, she was quite insensible, but unable to speak, or swallow, the tongue being greatly swollen and very painful, having been severely bitten during the fits.

On the 18th, cerebral symptoms being superinduced by the belladonna, it was discontinued, and general stimulants with appropriate local treatment, were directed. She convalesced speedily, and on the 27th was discharged well.

CASE IV.—Julia Kavanagh, aged thirty-eight; second pregnancy; admitted December 6th., 1869, brought in from North Union, where she had had a great number of epileptiform fits during the entire time of labour. Shortly after her admission she was delivered naturally of a living male child, weighing 7½ lbs., having been twenty-six hours in labour. Ten minutes after the expulsion of the placenta she had a violent epileptiform fit, which was checked by cold affusions, sinapisms,

and blisters. She was then put on  $\frac{1}{2}$ -grain doses of extract of belladonna, had no return of the seizures, and made a good recovery.

CASE V.—Julia Ward, married, aged twenty-five; first pregnancy. First seen at 11 a.m., April 5th, 1870. At this time she was completely insensible. The friends stated that she was seized with convulsions at 5 a.m., and since that time had had three. She rallied after the first, and conversed rationally, but since the second she had been comatose. A slight contusion was noticed above the right eye, caused by a fall at the access of the first paroxysm. Had passed urine involuntarily. Upon vaginal examination, the os uteri readily admitted the end of the finger. She was taken to the hospital, and had a convulsion while in the cab. On admission, comatose; slight œdema of lower extremities; urine drawn by catheter, and found loaded with albumen. 11 30 a.m.—Ordered

R Calomel,	-	gr. v.	R Spt. terebinth,	℥j.
Pulv. jalap. co,		gr. xv.	Tr. assafœtida,	℥j.
Ft. bolus.			Gruel,	- oj.
			Ft. enema.	

To be given at once.

Slight operation from enema. Paroxysms continued at intervals of about twenty minutes. The convulsions were general, epileptiform in character, and about one minute in duration.

At 1 50 p.m., ordered repetition of enema, sinapisms to calves of legs, and cold lotion to the head. The enema was not at all retained, a paroxysm coming on while it was being administered. About fifteen minutes after, slight action of bowels. During the afternoon the pulse was 90 during the interval of the convulsions, and 120 immediately after a paroxysm. After 1 30 p.m., there was continuous slight convulsive action during the intervals of the paroxysms. At 5 p.m., the administration of chloroform was commenced, and continued until 8 20 p.m.; during this time the convulsive action of the muscles ceased, but the paroxysms were unmodified in their character or duration, and occurred at average intervals of twenty-five minutes, 3jjj. 3j. of chloroform were administered. At 9 p.m., the os being about the size of a half-crown, and dilatable, the long forceps were applied, and the patient was delivered of a dead female child (the head was presenting in the third position). The placenta came away in five minutes. The uterus contracted well after delivery, and no hæmorrhage followed. Convulsions continued after delivery, at average intervals of about twenty-minutes. At 11 35 p.m., Dr. Johnston ordered an enema of one grain of aqueous extract of belladonna, in two ounces of beef-tea, sinapisms repeated. Enema repeated 12 30 a.m., April 6th, sinapism to back of neck at 12 40 a.m., pulse 140. Convulsions still continuing at brief intervals. Enema repeated at 3 30



a.m., and again at 6 30 a.m.; sank at 10 20 a.m. At no time since first seen had she been conscious.

*List of the Paroxysms.*

April 5th. Commenced at 5 a.m. Three before 11 a.m			
No.	Time	No.	Time
4	11 30 a.m.	29	12 25 a.m.
5	12 15 p.m.	30	1 0 a.m.
6	1 0 p.m.	31	1 15 a.m.
7	1 45 p.m.	32	1 28 a.m.
8	2 0 p.m.	33	1 55 a.m.
9	2 40 p.m.	34	2 20 a.m.
10	3 10 p.m.	35	2 37 a.m.
11	3 30 p.m.	36	2 55 a.m.
12	4 5 p.m.	37	3 30 a.m.
13	4 25 p.m.	38	3 48 a.m.
14	4 45 p.m.	39	4 15 a.m.
15	6 15 p.m.	40	4 35 a.m.
16	6 40 p.m.	41	5 5 a.m.
17	7 0 p.m.	42	5 20 a.m.
18	7 25 p.m.	43	5 40 a.m.
19	8 0 p.m.	44	6 8 a.m.
20	8 20 p.m.	45	6 25 a.m.
21	8 45 p.m.	46	6 47 a.m.
Delivered at	9 0 p.m.	47	7 25 a.m.
22	9 40 p.m.	48	7 50 a.m.
23	10 10 p.m.	49	8 20 a.m.
24	10 25 p.m.	50	8 45 a.m.
25	10 50 p.m.	51	9 5 a.m.
26	11 15 p.m.	52	9 20 a.m.
27	11 50 p.m.	53	9 50 a.m.
28	April 6th. 12 10 a.m.		

*Post-mortem* examination at 3 45 p.m., April 6th. Purple discoloration of dura mater, an inch and a half in length, and one inch in width, at a point corresponding to the union of the sagittal sutures with the coronal; surface of brain congested. Clots in posterior portion of superior longitudinal sinus, and colourless fibrin throughout the whole extent of the sinus; about 3ij. of serum in right ventricle.

Uterus rising to lower margin of umbilicus, and containing several clots. Kidneys apparently normal on section. A small quantity of pus found in the pelvis of each. Local peritonitis in region of left kidney.

CASE VI.—April 18th. B. W., aged thirty, first pregnancy; was delivered of a living female child, after a labour of sixteen hours. A short time after the commencement of labour she was attacked by asthenic convulsions, after which she became unconscious. The fits continued to recur at shortened intervals during the entire time of labour. All the usual remedies—cold affusion, purgatives, counter-irritants, &c.—being, of course, resorted to, though without benefit. The placenta was retained by want of uterine action for two hours, and shortly after its expulsion she sank, and died in a comatose state.

CASE VII.—M. K., aged thirty-five, was delivered of her sixth child, a healthy male, on May 14th, after a natural labour of twenty-four hours. The placenta followed almost immediately, and ten minutes after its expulsion she was attacked by asthenic convulsions, having only one fit however. This lasted for seven minutes, and was checked by cold affusions and sinapisms to the calves of the legs. After the seizure she remained unconscious for some time, but had no return of the attack, and made a good recovery.

CASE VIII.—Within the last month, through the kindness of Dr. J. Byrne Power, I had an opportunity of seeing a very interesting case of puerperal eclampsia in a patient of his—a lady, aged about thirty-two, who was attacked with asthenic convulsions immediately before labour. It was her third pregnancy, and several years had elapsed since her last confinement. The convulsions commenced about midnight, April 17th, when Dr. Power was sent for, and resorted to all the measures that could be employed to arrest the disease. When I saw her, about 5 a.m., she was completely unconscious, and, despite the judicious treatment which had been employed by Dr. Power, the fits recurred about every twenty minutes with increasing violence. The convulsions were general, but more marked on the right side. The os was still undilated and rigid, but after some time we were able to introduce, first one large-sized Barnes' dilator, and subsequently a second; but, finding it impossible to overcome the rigidity sufficiently to effect delivery, we were ultimately obliged to incise the os sufficiently to allow version to be performed, and were compelled to complete the operation with the forceps, as the os contracted so firmly after the shoulders had passed as to prevent delivery being otherwise accomplished. The child, a male, was still-born. After the operation the uterus contracted firmly, the placenta was expelled immediately, and there was no hæmorrhage. Half an hour subsequently she had another seizure, and at intervals seven other attacks. She never recovered consciousness, and died five hours after.

DR. H. RINGLAND said there seemed to be a difference of opinion as to what puerperal eclampsia really was. Some maintained it was apoplectiform in character, while others held its similarity to epilepsy, and many regarded it as purely hysterical—a view which he understood Dr. Madden to hold, and with which he entirely disagreed.

The PRESIDENT said that Dr. Madden had not expressed any such opinion.

DR. MADDEN said Dr. Ringland had misunderstood him.

DR. RINGLAND proceeded to say that at the Coombe Lying-in Hospital the majority of the staff held to the epileptic theory. The mode of invasion, the spasm of the glosso-pharyngeal apparatus, the tonic and clonic nature of the subsequent convulsions, to say nothing of the state of semi-coma the patient falls into after the attack—all tended to prove the correctness of that opinion. He had never met with a case of real eclampsia without the presence of albuminaria. The treatment of one of Dr. Madden's cases must, he thought, be considered questionable, or, at any rate, not one to be adopted without very mature deliberation—viz., the incision of the os uteri. To teach that practice would be hazardous, and in all the cases he (Dr. Ringland) had seen—over fifteen in number—they all answered freely to the usual treatment—bleeding, tartar emetic, and opium in some, and chloroform in others. In one case lately, on the advice of his colleague, Dr. Kidd, he used morphia and atrophine hypodermically with marked success. He thought that craniotomy need never be resorted to as a means of delivery in puerperal eclampsia.

DR. ATTHILL said :—Dr. Madden's elaborate paper takes in the causation, preventive treatment, and active treatment, if I may use the expression, of convulsions. Now, as to the causation, in certain cases we are not able to trace it; but this is proved beyond all manner of doubt, that in the immense majority of cases, in at least 90 per cent., albuminous urine and puerperal convulsions are synonymous terms. Dr. Ringland spoke of puerperal convulsions as being epileptic. With this view I entirely disagree. During twenty-five years I never saw a case of epileptic convulsions occurring in a puerperal patient. I never saw convulsions occurring in labour in a person previously subject to epileptic fits; nor did I ever see a case of puerperal convulsions that could be classed with any propriety as epileptic. I again repeat that albumen in the urine and puerperal convulsions are, as *a rule*, synonymous terms. There is, in addition to that great class, another class which, for lack of a better term, we must call hysterical—that is, the convulsions depend on some untraceable form of reflex irritation, causing convulsions which, producing no pathological condition, renders it impossible for us to trace the cause. These cases are few. As to the previous treatment, where we have well-defined symptoms—the frontal head-ache, the flushed face, œdema of the feet and of the upper extremities—we know that spare diet, exercise, purgatives, and blood-letting will generally avert the threatened attack. These symptoms do not occur in those rare forms of reflex irritation to which I have alluded. We now come to treatment to be practised at the time. Blood-letting is, in a certain class of cases, of great benefit; but I think blood-letting may be practised injudiciously. There are doubtless a certain number

of patients that will bear blood-letting, even to the amount of 40 or 50 ounces, pretty well, but I should be afraid to go to such an extent. But it is evident, from the fact of albumen in the urine being such a constant concomitant in these cases, that the kidney, not the head, should be treated, and therefore more benefit may be derived from cupping over the loins than will follow from venesection in the arm. The cold affusion I have no great faith in. It may produce a certain effect for the time, but it will have no permanent effect in preventing the recurrence of the convulsions. Next to blood-letting and cupping over the loins, I place faith in active purgation. As to chloroform, it is most valuable as a preventive; I have stopped the convulsions for eight hours consecutively, and, although they recurred when it was withdrawn, that long remission was in itself no small benefit. During those eight hours the os had dilated to such an extent as to enable me to effect delivery, and that was a great matter. Blood-letting, purgatives, and chloroform, I regard as our sheet anchors. Tartar emetic is uncertain and comparatively slow in its effects. With respect to the practice of cutting the hair, bleeding the neck, and applying ice to the head, it is often useless. The head, in a great majority of cases, is affected only secondarily; and unless you can remove the real cause of the convulsions—that is, the blood-poisoning dependent on congestion of the kidney—the treatment directed to the head will be altogether useless. Dr. Madden has told us that in some of his cases the kidneys were not diseased; but you may have albumen in the urine without any disease of the kidneys. The congestion of the kidney may be purely mechanical, depending on pressure on the renal veins, and you may have a healthy kidney, and yet find albumen in the urine. Our great object is to effect delivery; but I should hesitate to divide an undilated os to enable me to introduce my hand into the uterus. The incision would, in all probability, be so increased by the subsequent dilatation which would occur during the extraction of the child as to cause a rent that might endanger life. It does not follow that because one case is successful, the practice is therefore sound or free from danger. Dr. Madden has said that in many cases the head is low in the pelvis, while the os is undilated, and therefore that the division of labour into a first and second stage was not in such case correct. It does not follow, however, that because the head is low, we can with safety effect delivery. Dr. Denman long ago laid down the axiom that a tedious first stage of labour was not dangerous—an axiom that every practitioner here will fully endorse—that is, of course, excepting such cases as hæmorrhage and convulsions; but to reverse the rule, and say you may hasten the first stage by any forcible means you may decide on, if only the head is in the true pelvis, seems to me not only illogical but dangerous, and I would therefore protest against the incision of an undilated os and forcible extraction of the child.



The PRESIDENT—I was very much struck with Dr. Madden's paper. Without going into his reasoning, his practice appears to me to be very sound, with the exception of his using the knife so freely. He seems not to have lost sight of the old landmarks in dealing with these cases. It is a remarkable fact that every one who has spoken this evening has dwelt on the importance of depletion. It is very satisfactory to find that, in investigating the disease referred to, and in tracing its connexion with albumen in the urine and blood-poisoning, we do not lose sight of the practice on which the lives of our patients depend as confirmed by experience. For many years every one thought puerperal convulsions arose from determination of blood to the head, and considered that depletion was the only means of preventing immediate death; and I believe whether there be blood-poisoning or not, whether, as we see in jaundice, the bile is carried back into the blood-vessels and circulated in the brain, or whether, as we see in some forms of the disease, the same poisoning is caused by want of action in the kidneys, still there is in a large majority of cases a state of congestion of the brain by serum, or blood or bile, and I believe the only way of preventing immediate fatality in those cases is by depletion. In the last case of convulsions I was called to see, the patient had lain for several hours with eclampsia, fit occurring after fit. The woman was apoplectic; in the intervals the lancet was used, and the patient recovered her consciousness, although she had been eight or nine hours unconscious. Time was then given for treatment, and the woman delivered of a living child, without any return of the fits. I cannot look back on any one case of convulsions where I used depletion that I had reason to regret it. I think, therefore, Dr. Madden's insisting on this point is of vital importance, and shows that we have not lost sight of our reliable practice on investigating the true nature of the disease. Now as to chloroform, there seems to be some discrepancy of opinion on the subject. I look on chloroform as unreliable as a curative means of convulsions; but I agree with my friend, Dr. Atthill, who thinks it a valuable means of allaying irritability and lessening the fits until your treatment gets time to tell; and his suggestion for dealing with the kidney is a very sound one pending the progress of the disease. Chloroform is useful to allay irritation, unless, perhaps, when the convulsions depend upon reflex irritation without organic or functional disease, but not curative. As to Dr. Madden throwing over the different stages of labour, I think he must have been converted by Dr. Power's book, published about 1820, in which he expressed the opinion that pain in labour was entirely unnecessary, and a phenomenon to be entirely avoided and prevented—maintaining that the uterus should expel its contents without any necessity for undergoing the different stages of labour, but simply as the rectum or the bladder would throw off its contents. His book was a very clever one; but, as

chloroform had not then been used in labour, I doubt whether he made many converts to what would then have been esteemed the anti-Scriptural view of "labour being without pain." I am afraid Dr. Madden cannot do away with the stages of labour. Although not an advocate for the free use of the knife, I do not deny that it may be occasionally necessary, but we should be very cautious before we have recourse to it. Formerly the rule was that no one should deliver a woman in convulsions unless he was imperatively obliged to do so, and the impression then was that the irritation produced by forced labour of any kind was such that the likelihood of a fatal termination was increased by it, but especially by turning. In my mind that doctrine was carried to an unjustifiable extent. I therefore took a line of my own, and did deliver convulsive patients contrary to the rule. The apprehensions of the earlier practitioners on the dangers attending forced deliveries in eclampsia should not, however, be disregarded. In proof of this I could lay my hand upon men grown to manhood who were brought into the world when the mother was in a state of convulsion, and when the mothers did not survive the operation. I merely mention this to suggest caution in dealing with these cases. It seems to me that if you cannot prevent or lessen the frequency of the fits, or relieve congestion of the brain by treatment, depletion, tartar emetic, or chloroform, then you are justified in having recourse to forcible extraction; but from my own experience I do not think you are justified in having recourse to it under other circumstances. There is one other point as to aspersion which I wish to remark upon. It was made light of, but I think it is useful in postponing or lessening the frequency of the fits. We are, then, justified in bleeding, in employing counter-irritation over the kidneys, in using chloroform (and I have seen magical effects from chloroform), and in aspersion; and the combination of these is the sound practice to adopt without reference to the *rationale* of the disease at all.

DR. H. KENNEDY said that many years ago he was employed to make a *post-mortem* of a female who died of this disease. She had been largely bled, and on making an examination of the brain he found one side of it ploughed up by apoplexy.

DR. ATTHILL hoped it was not supposed that he denied the fact of apoplexy in these cases. He believed the effusion of the serum was a consequence of the fits, not the cause of them.

The PRESIDENT observed that in death from convulsions they invariably found evidence of effusion.

DR. MADDEN briefly replied. The observations made by Dr. Ringland

about epileptic convulsions had been sufficiently answered by Dr. Atthill. Dr. Atthill said that albumen in the urine and convulsions were synonymous terms. Now he (Dr. Madden) had seen sixteen cases altogether, and out of these a small proportion—certainly not more than one-half—had albuminous urine, and on the other hand women with albuminous symptoms frequently escaped convulsions. Dr. Atthill regarded the application of the knife as a hazardous remedy. He agreed with him that it was a desperate remedy, but he ventured to remind him that the disease was a desperate one, and that if the woman were not delivered she would die. The only cases in which he used the knife were where all other means of effecting the delivery had been fairly tried and had failed—that is, the delivery of a living child from a living mother. With regard to cupping over the loins, it sounded very well, but it would be found no easy matter to cup a woman in puerperal convulsions; besides which the practitioner did not generally carry a cupping apparatus in his obstetric bag. It must be remembered that although the starting-point of the disease might be in the kidney, as Dr. Atthill contended, yet, when convulsions set in, it was the spinal centres that were affected, and cupping over the loins would have but little effect then. If, indeed, the cupping were effected before the convulsions began, it might cut short the disease. The President had expressed his views on depletion. The cases he had seen recover were invariably cases in which blood-letting had been resorted to. Tartar emetic was useful, if there were time, but if the convulsions were increasing in frequency, and the life of the patient in danger, then he would resort to the knife; but, as already stated, he had never resorted to that method until tartar emetic and all other means had been tried and had failed. He did not mean to do away with the necessity of dividing labour into stages as a safe landmark for students; but where they were sailing, as it were, between Scylla and Charybdis, they had not time to pay attention to these theoretical divisions.

The PRESIDENT had omitted to mention one point which he thought most essential, and that was the almost invariable presence of head-ache in the last months of pregnancy in cases of puerperal convulsions. Whenever, then, an obstetric practitioner met with a case in an advanced stage of pregnancy in which the patient complained of head-ache, let him examine the urine, and look forward to the probability of convulsions, and at once treat the kidneys; deplete locally, and, if necessary, generally; administer saline aperients and alteratives, &c., and thus prevent this alarming disease.

*Case of Tetanus following Abortion.* By M. A. BOYD, L.R.C.S.I.,  
L.K.Q.C.P.I., M.R.I.A.; Surgeon to St. Michael's Hospital, Kings-  
town.

GENTLEMEN,—The case which I beg to lay before you to-night is one of tetanus following abortion at the third month, and, from its rarity, I deem it of sufficient importance to bring under your notice.

On the 3rd of last month I was sent for to see a thin, anæmic woman, suffering from abortion in her first pregnancy, produced by a fall in reaching to a clothes line. On examining her I found the ovum protruding from the os; hæmorrhage, which had been pretty free during the night and morning previous, having almost entirely ceased. I made an attempt to remove the ovum with my finger, but failed in doing so, a part only coming away. As the hæmorrhage again returned during my endeavours to remove it, I plugged the vagina before leaving, and ordered ergot, with gallic acid and opium, to be given every third hour.

On the following morning I removed the plugging, and, as the hæmorrhage had almost entirely ceased, did not re-introduce it, but continued the ergot and gallic acid. Next day I was informed, on visiting her, that the remains of the ovum had entirely come away, attended with some hæmorrhage; but the fragments I found were so mixed up with blood-clots, and so adherent to articles of clothing, that I did not feel satisfied as to the whole of it being expelled. On examining, however, I found the uterus collapsed, and no evidence on digital pressure that its cavity contained any of its remains.

She continued to progress favourably, with no recurrence of hæmorrhage, till I saw her next day. As some pain and tenderness were then complained of over the uterus, I ordered a linseed poultice to the abdomen, with 20 grains of the hydrate of chloral at night.

As she continued to do well the two succeeding days, taking nourishment, and suffering no pain or weakness, I did not consider it necessary to see her.

On the morning of the 9th, the sixth day from the time I first saw her, I was sent for early to visit her. On arriving I found that she had spent a restless night, and had several fainting fits; her pulse was feeble and quick; her deglutition difficult and painful, with a sense of choking, and stringy mucus, which she could with difficulty get away, filling up the back of the pharynx; the masseter muscles were rigid, and the teeth could, with difficulty, be separated to a distance that would admit the index finger. I found these symptoms set in five hours previously, after awaking from a short sleep. The peculiar expression of face, aptly termed the risus sardonicus, was also well marked, but at this time there was no opisthotonos, or rigidity of neck, or of any muscles, except those of the face and pharynx.



I immediately admitted the patient to hospital, and ordered 25 grains of hydrate of chloral to be given every four hours; and taking into account her weak pulse, and the recent loss of blood, ordered beef-tea and wine to be given freely every third hour.

On visiting next morning, the 10th, I found she did not enjoy more than ten or fifteen minutes' sleep, at intervals, during the night. Opisthotonos had also set in, the spasms of which, on making the slightest movements, were most painful and intense, and generally coming on after dozing for a few minutes. The muscles of the lumbar region and extremities were, however, from beginning to end entirely unaffected. Her pulse was also up to 138 in the minute, but stronger than on the previous day. I increased the dose of chloral to 30 grains every fourth hour, but its administration caused such a feeling of burning in the throat, though largely diluted, and induced such paroxysms of painful spasm, that I had to change my hand, and order its administration per rectum—35 grains being given in this way, at the same interval, in some warm beef-tea.

I again made a digital examination per vaginam to ascertain lest by any chance a portion of the secundines might still be remaining in the uterus, and causing the irritation which led to the trismus, but I found the uterus was fast regaining its natural size, the os almost closed and the cervix elongated, and a slight leucorrhœal discharge coming away; the sero-sanguineous one following the expulsion of the ovum having, I should mention, ceased prior to the supervention of the trismus symptoms.

On the morning of the 12th I found she had spent the last night much easier than the previous one, continuing in a state of stupor from time to time from the effects of the chloral, and during which the muscles of the face and neck were considerably relaxed, but spasm generally returned to some extent when she was roused to take nourishment, and her pulse had risen to 140 in the minute, but respiration continued, as it had from the beginning, quite normal.

I again made a vaginal examination to satisfy myself as to the condition of the os and uterus, and not being perfectly assured, from the previous imperfect examinations, but some portion of the ovum might still be remaining, decided on dilating the os to explore the uterine cavity. I accordingly introduced four pieces of sea-tangle, varying in size from that of a No. 6 catheter to a No. 12—having, previous to the operation, placed the patient under the influence of chloroform. The dose of chloral administered per rectum was now also increased to a drachm every fourth hour. As little or no nourishment could be taken, nutrient enemata were almost entirely substituted, and the occasional inhalation of chloroform when the spasms were severe. During the night two attacks of general convulsions set in, lasting about a minute;

there was no recurrence during the latter part of the night, and several hours' tranquil sleep supervened.

On the morning of the 13th I removed the sea-tangle tents, and I was enabled to introduce two fingers and thoroughly explore the cavity of the uterus and satisfy myself that no portion of the ovum whatsoever remained. The pulse had now risen to 150 in the minute, and respiration was quick and laboured. Towards evening the patient seemed worse, and lividity of the lips, with increased quickness of respiration, showed itself—a sure evidence of congestion of internal organs, while increasing stupor and complete relaxation of all spasm indicated the last act of the painful drama. She continued in this state during the night, occasionally waking up to consciousness when the enemata were being given, which now consisted of brandy and ammonia, but towards morning became more and more prostrate, and died apparently from the combined effects of exhaustion and congestion of the lungs on the sixth day from the commencement of the tetanic symptoms.

The retention of a foreign body in the uterus, such as a detached ovum, or fragments of it, which is assigned by most writers as the principal cause of tetanus following abortion, could not be looked on as the true source in this case, as the dilatation of the os and exploration of the uterine cavity set that idea completely aside. I am inclined to attribute it to the irritation which the brain must have suffered from deprivation of blood in an already anæmic subject, for we know how prone such subjects are to convulsions, an allied spasmodic nervous affection after even very trifling hæmorrhage, and it is difficult to say where the line of demarcation can be drawn between the irritation that gives rise to the clonic spasms of convulsions in one case and the tonic ones of tetanus in another.

As to the cause of death in this disease, pain from spasm of the muscles, which is assigned as one of the principal ones, could not be said to be so in this case, as from the time she was completely under the influence of the chloral, when large doses of it were given, upwards of a drachm every fourth hour, or about one ounce in the twenty-four, she never complained of pain, and the stupor which it induced was accompanied by considerable relaxation of the muscles of the neck and jaws; it was not the comatose stupor that large doses of opium produces, and from which a patient can with difficulty be roused, but a state of perfect deep sleep from which the patient was easily roused, and which rather resembled the heavy sleep of a person intoxicated than one of coma.

Here I may advert to the large quantity administered—nearly an ounce in the twenty-four hours; the first doses given—namely, 20 grs. every fourth hour—produced neither sleep nor freedom from pain till the dose was increased up to 35 and 40 grs. in the same interval. Whether or not the system was more tolerant of the drug, owing to the

condition of pain and spasm, just as patients are more tolerant of large doses of opium under the same circumstances, I am not prepared to say, but I was principally induced to try it as a remedy in this disease from its action as a paralyzing agent on the vaso-motor nerves, more particularly those of the head and face, as shown by Dr. Ludwig Kirn in a paper translated from the German, and appearing in the June number of *The Practitioner* for last year. I believe remedies, to be of any service in this disease, must act through the agency of these nerves alone, and our best authorities agree in looking on the vagus and sympathetic from reflected irritation conveyed to them through the medulla oblongata as the *locale* of the disease, and a remedy like hydrate of chloral, which acts directly on them, the most suitable drug to be administered.

It appeared to me, also, that an agent of this kind is the only one from which we are to expect any good results in treatment, and not from any of the host of anti-spasmodic remedies, so called, which merely act by relaxing the muscles alone, not through the agency of the nerves, but as depressents of the general system—results which we could as easily obtain by any emetic or a prolonged warm bath.

DR. MACSWINEY said, that it had been observed during the previous discussion that desperate cases required desperate remedies. The occurrence of tetanus under the circumstances mentioned by Dr. Boyd was of such a desperate character that almost any remedy that could be tolerated would be justifiable in its treatment. He thought, were it not for this explanation, the enormous doses of chloral hydrate given in this case could not be justified. He believed the doses given by Dr. Boyd were the largest on record that had been given, either by the mouth or the rectum. He did not imply any censure on the treatment, for he was certain the desperate character of the disease would be held to justify a recourse to any means, however heroic, that would hold out any faint hope of being serviceable.

DR. ATTHILL said that he had never seen a case of puerperal tetanus. He agreed that Dr. Boyd's case was a valuable one, and clearly detailed. Several such cases had been recorded lately in the medical journals, and there was one suggestion which attracted his attention, and which he was determined to put in practice if he should ever meet with a case of the kind, and that was transfusion—either transfusion of blood alone, or transfusion of blood with a combination of such medical agents as might be considered desirable. He did not object to the treatment adopted by Dr. Boyd. As yet they knew of no remedy that would arrest the progress of tetanus.

DR. MADDEN should not like this discussion to go forth without a word

of protest against the giving of such enormous doses of such heroic a remedy as hydrate of chloral. The treatment was, no doubt, justifiable in the particular case brought before them. He (Dr. Madden) was one of the first who read a paper in that Society on hydrate of chloral, and he then mentioned an extraordinary case in which he gave 30 grains by the rectum until 90 grains had been administered in the day, and he thought himself exceedingly fortunate that his patient survived; but he certainly never heard or read of a case in which doses amounting in the aggregate to an ounce of a drug so powerful, were administered to a patient before in the same space of time.

DR. BOYD—As to taking a large quantity of chloral, I may state that I have a patient at present for whom I ordered a two-ounce bottle of syrup of chloral, a teaspoonful to be given every two hours till sleep had been procured, the whole of which she ultimately took; and she was then in the habit of sending out for another two-ounce bottle, and took the two, amounting to  $\frac{3}{4}$  of an ounce of chloral, and this she had been in the habit of doing for three weeks or a month.

DR. DARBY (who, in the absence of Dr. Kennedy, now occupied the chair) said he knew a first-rate practitioner in this city who, when the drug first came out, gave 90 grains in one dose. The facts of that case, he believed, had been published. He himself on one occasion, in puerperal mania, when it was impossible to procure sleep by other means, gave 60 grains in one dose. He was under the impression that they might venture to give a larger dose by the rectum than by the mouth. He knew of cases in which persons of their own *mere notion* took syrup of chloral to a large extent, but perhaps not to so large an amount as was given by Dr. Boyd. At the same time he advised them to be very cautious in administering chloral. He generally gave 8 or 10 grain doses; and even with these doses, when repeated two or three times in succession, the individuals frequently complained of burning fauces and other unpleasant symptoms.

The Society adjourned to the 13th June.





# INDEX

## TO THE FIFTY-SEVENTH VOLUME.

---

- "A Lame Story," 401.  
 Abdomen, incised wound of, 205.  
 Abdominal aneurism, 299.  
 Abortion, case of tetanus following, 583.  
 Abscess, renal, Dr. Peebles' case of, 338.  
 Accidental deaths, 197—hæmorrhage, 187.  
 Alkaloids, 146.  
 Althaus, John, on electricity, *Rev.*, 444.  
 Amputation of penis at pubes, 222.  
 Amputation by Esmarch's method, 387.  
 Amyl, nitrite of, 62.  
 Aneurism of abdominal aorta, 299.  
 Animal physiology. By Dr. J. Cleland, *Rev.*, 316.  
 Ankylosis of the hip-joint. Dr. Bennett on, 489.  
 Anomalous labour. Mr. C. D. Moutray on a case of, 343.  
 Apomorphia, 59.  
 Apoplexy, 191.  
 Aspiration in empyema, 15.  
 Atlas of skin diseases, *Rev.*, 447.  
 Athill, Dr. Lombe, on various uterine tumours, 174—on the use of the perchloride of iron in *post-partum* hæmorrhage, 346—malignant tumour of uterus, 381.  
 Barnett, Dr. Richard, on fractures of the inferior maxilla, 397.  
 Barton, Dr. J. K., on a case of medullary cancer, 92—osteitis, 96.  
 Beard, G. M., electro-surgery, *Rev.*, 445.  
 Belladonna poisoning, 139.  
 Bellamy, Edward, guide to surgical anatomy, *Rev.*, 442.  
 Bennett, Professor, case of incised wound of the abdomen, 205—stricture of the urethra, 378—true ankylosis of the hip-joint, 489.  
 Benson-Baker, Dr., on croton-chloral, 145.  
 Blanc's notes on cholera, *Rev.*, 417.  
 Boyd, Dr. M. A., on a case of tetanus following abortion, 583.  
 Bright's disease, cases of, 478, 487, 488.  
 Bromal, 63.  
 Bromide of potassium in epilepsy, 163.  
 Bromoform, 62.  
 Browne, Dr. Crichton, on nitrite of amyl in epilepsy, 145.  
 Budd, Dr. Wm., on typhoid fever, *Rev.*, 311.  
 Caisson disease, Smith's, *Rev.*, 448.  
 Calculi, biliary, a cause of jaundice, 209.  
 Cancer, Dr. Barton's case of medullary, 92.  
 Carbolic acid and zymotic disease, 138.  
 Carcinoma of liver, 303, 382—colloid, of ovary, 370.  
 Cerebral tumour, optic nerve in, 538.  
 Cerebro-spinal meningitis, Dr. James Little's case of, 266—Dr. Grimshaw's case of, 375.  
 Charles, Dr., case of gastric ulcer, 390.  
 Chloral, 63, 140, 141, 585, 586, 587.  
 Chloralism, 142.  
 Chloroform, 61, 141, 193.  
 Cholera, notes on, Blanc's, *Rev.*, 417.  
 Cinchona alkaloids, 149.  
 Cirrhosis of lung, 479.  
 Clarke, Dr. W. Fairlie, on diseases of the tongue, *Rev.*, 317.  
 Cleland, Dr. John, on animal physiology, *Rev.*, 316.  
 Climate and vital statistics of Tasmania, by Dr. J. W. Moore, 151.  
 Colles, Mr. Wm., on injection of hæmorrhoids with perchloride of iron, 585.  
 Colloid carcinoma of ovary, Dr. Kidd's case of, 370.  
 Conium, Dr. H. Kennedy on, 67.  
 Constipation in enteric fever, 377.  
 Convulsions, 190.  
 Convulsions, hysterical, 272, 287.  
 Convulsions, puerperal, 560.  
 Corley, Dr. A. H., on fractures of the skull, with special reference to operative interference, 305.  
 Craniotomy, 185.  
 Croton-chloral, 64, 142.  
 Current literature of insanity, *Rev.*, 253.  
 Cystic disease of the ovary, Mr. Porter's case of, 94.  
 Darby, Dr., on uterine hydatids, 464.

- Deaths, in 1873, in Rotunda Hospital, 193.  
 Delirium tremens, case of, 394.  
 Dermatology, lectures on, Wilson's; *Rev.*, 414.  
 Dieulafoy's treatise on pneumatic aspiration, *Rev.*, 440.  
 Dislocation of hip-joint, 388.  
 Drainage-tube after ovariectomy, 536.  
 Drysdale's granular cell of ovarian fluid, *Rev.*, 450.  
 Ear, diseases of the, Roosa's treatise on, *Rev.*, 435.  
 Elbow, excision of, 385.  
 Electricity, works on, *Rev.*, 444—hand-book of medical, Tibbits, *Rev.*, 448.  
 Elephantiasis græcorum, *Rev.*, 135.  
 Emetin, 59.  
 Empyema, case of, 15.  
 Enteric fever, Dr. William Moore's case of, 94—ulceration of ileum in Dr. Foot's case, 296—perforation in, 297—peritonitis without perforation, 301—Dr. Stokes' case of, 483.  
 Epilepsy, nitrite of amyl in, 145.  
 Epilepsy, 191.  
 Ergotin, 60.  
 Extension apparatus for fractured femur, 1.  
 Eustachian tube, recent works on the, *Rev.*, 406.  
 Excision of knee-joint and its after-treatment, 97—elbow, 385—knee, 486.  
 Fagan, Dr., on noma pudendi, 388.  
 Femur, fractured, treated by extension apparatus, 1.  
 Fever, cerebro-spinal, 375 — intestinal lesions of enteric, 94, 377—intestinal hæmorrhage in, 549.  
 Fever, lectures on, by Dr. Stokes, *Rev.*, 510.  
 Fevers of Great Britain, Murchison on, *Rev.*, 48.  
 Finny, Dr. J. M., on rest, 108.  
 Fitzgerald, Dr. C. E., on the optic nerve in cases of cerebral tumour, 538.  
 Fitzmaurice, Mr. Robert, practical observations, 13—remarks on small-pox, 13—notes of a case of empyema, 15.  
 Flax-spinners, lichenoid eruption on, 507.  
 Foot, Dr. Arthur Wynne, select clinical reports, 209—cases of jaundice, 209—cirrhosis of liver, 219—hæmorrhage and ulceration of ileum in enteric fever, 296—perforation of ileum in enteric fever, 297—æso-phagismus, 327—constipation and ulceration in enteric fever, 377—carcinoma of liver, 382—pneumonic and intestinal phthisis, 485.  
 Forceps, 182.  
 Fracture of os calcis, Mr. Porter's case of, 94—patella, Dr. T. E. Little's case of, 207—skull, operative interference in, Dr. Corley on, 305.  
 Funis, prolapse of, 190.  
 Garretson's oral surgery, *Rev.*, 415.  
 Gastric ulcer, Dr. Charles' case of, 390.  
 Gastro-duodenal catarrh, 219.  
 Gonorrhœa, latent, in the female. By Dr. Emil Næggerath, *Rev.*, 324.  
 Gordon, Dr., hæmorrhagic sarcoma of pleura, 452.  
 Gout, rheumatism, and rheumatic gout. By Austin Meldon, *Rev.*, 264.  
 Granular cell of ovarian fluid, Drysdale's, *Rev.*, 450.  
 Grimshaw, Dr., a case of cerebro-spinal fever, 375—hepatic abscess, 476.  
 Gross, Samuel D., a system of surgery, *Rev.*, 20, 226.  
 Guarana, 58.  
 Hæmatemesis, fatal, 219.  
 Hæmorrhage, accidental, 187—*post-partum*, preventive treatment of, 75, 188—Dr. D. Johnston on, 392—unavoidable, 188—intestinal, in fevers, 549.  
 Hæmorrhoids, injection of, with perchloride of iron, 505.  
 Hamilton, Mr. John, lectures on syphilitic osteitis and periostitis, *Rev.*, 315.  
 Hand deformed by burn, 302.  
 Hayden, Dr., on bromide of potassium in epilepsy, 163—coincident mitral and tricuspid stenosis, 459—chronic renal disease, 478—Bright's disease and œdema of the glottis, 487—amyloid degeneration of the kidney, 488.  
 Hemlock, further observations on the use of, by Dr. H. Kennedy, 67.  
 Hip-joint, reduction of dislocation of, 388—true ankylosis of the, 489.  
 Hoffmann's manual of chemical analysis, *Rev.*, 446.  
 Hogg, Mr. Jabez, on skin diseases, *Rev.*, 134.  
 Homœopathic pilules, 136—camphor, poisoning by, 137.  
 Hughes, Dr. J., cirrhosis of lung, 479.  
 Humphry, Prof., *Journal of Anatomy and Physiology*, *Rev.*, 407.  
 Hydatis of uterus, Dr. Darby's case of, 464, 281.  
 Hysteria, Dr. MacSwiney's case of, 271.  
 Hysterical convulsions, case of, 287.  
 Infancy and childhood, diseases of, Dr. West on, *Rev.*, 408.  
 Insanity, and hospitals for the insane in Ireland, *Rev.*, 44—in its relations to crime. By Dr. Hammond, *Rev.*, 253—*American Journal of*, 1872, *Rev.*, 253.

- Intestinal lesions of enteric fever, 94, 377  
—hæmorrhage in fevers, Dr. H. Kennedy on, 549.  
Iodoform, 62.
- Jaundice, from impaction of calculus, 209  
—from passage of calculi, 214—  
from emotional causes, 217—  
from gastro-duodenal catarrh, 219.
- Johnston, Dr., report of the Rotunda  
Lying-in Hospital for the year 1873, 177.
- Johnston, Dr. David, on *post-partum*  
hæmorrhage, 392.
- Jordan, Furneaux, surgical inquiries, *Rev.*,  
443.
- Journal of Anatomy and Physiology,  
*Rev.*, 407.
- Kelly, Mr. Dillon, on small-pox at Mul-  
lingar, 6.
- Kennedy, Dr. Henry, on the use of  
hemlock, 67—intestinal hæmorrhage in  
fevers, 549.
- Kidd, Dr., a case of fibrous tumour of  
uterus, 281—colloid carcinoma of ovary,  
370—report on midwifery, 515.
- Kidney, dislocation of the, Dr. Peebles'  
case of, 338—horse-shoe, 391—amyloid,  
488.
- Klein's anatomy of the lymphatic system,  
*Rev.*, 424.
- Knaggs, Samuel T., M.D., lessons from  
general practice, 1.
- Knee-joint disease, 376.
- Labour, induced, 187—anomalous, 343—  
complicated, 385.
- Laryngeal phthisis, Dr. W. Moore's case  
of, 475.
- Lead, nitrate of, in onychia maligna, 145.
- Lectures on madness, by Edgar Shep-  
pard, M.D., *Rev.*, 253—syphilitic  
osteitis, by J. Hamilton, *Rev.*, 315—  
fever, by Dr. Stokes, *Rev.*, 510.
- Leg, the history of a bad, 401.
- Lessons from general practice by Dr.  
Knaggs, 1.
- Lichenoid eruption on flax-spinners, 507.
- Little, Dr. James, on morphia in cerebro-  
spinal arachnitis, 266.
- Little, Dr. T. Evelyn, on comminuted  
fracture of the patella, 207.
- Living, Dr. Robert, on elephantiasis  
græcorum, *Rev.*, 135.
- Liver, cirrhosis of the, 219—carcinoma of,  
303, 382—abscess of, 476.
- Lungs and heart, laceration of, 300.
- Lupus-disease of the skin, Squire's, *Rev.*,  
447.
- Lupus erythematosus, 388.
- Lymphatic system, Klein's anatomy of  
the, *Rev.*, 424.
- Madden, Dr. More, on specimens of mole  
pregnancy and so-called uterine hyda-  
tids, 281—on puerperal convulsions,  
560.
- Mania, 192.
- Manual of chemical analysis, Hoffman's,  
*Rev.*, 446.
- Materia medica and therapeutics, report  
on, 136.
- Maxilla, fracture of inferior, cases of,  
397.
- Medical Society of the College of Physi-  
cians, Transactions of the, 57, 151, 266,  
327, 422, 538—Ulster, Transactions of  
the, 385.
- Medicine, Roberts' handbook of the theory  
and practice of, *Rev.*, 326.
- Medullary cancer, Dr. Barton's case of  
92.
- Methyl-strychnia, 58.
- Mitral and tricuspid stenosis, coincident,  
Dr. Hayden on, 459.
- Mole pregnancy, 281.
- Moore, Dr. John, on excision of elbow,  
385—abnormal presentation in labour,  
385—amputation by Esmaich's method,  
387—reduction of dislocation of hip-  
joint, 388.
- Moore, Dr. J. W., on the climate and  
vital statistics of Tasmania, 151.
- Moore, Dr. William, intestinal lesions of  
enteric fever, 94—laryngeal phthisis,  
475.
- Morphia in cerebro-spinal meningitis, 266.
- Moutray, Mr. C. D., on a case of anomal-  
ous labour, 343.
- Mullingar, report on small-pox at, 6.
- Murchison, Dr., a treatise on the continued  
fevers of Great Britain, *Rev.*, 48.
- M'Clintock, Dr. A. H., on the preventive  
treatment of *post-partum* hæmorrhage,  
75.
- MacLachlan and Stewart's pathological  
charts, *Rev.*, 48.
- MacSwiney, Dr., on a case of hysteria,  
271—on a case of rupture of the  
urethra and perineum in labour, 290.
- Nerve, optic, in cerebral tumour, 538.
- New Sydenham Society's Atlas, *Rev.*, 447.
- Nitrite of amyl, 62—in epilepsy, 145.
- Nixon, Dr., a case of peritonitis without  
perforation in enteric fever, 301.
- Nöggerath, Dr. Emil, on latent gonor-  
rhœa in the female, *Rev.*, 324.
- Noma pudendi, Dr. Fagan's case of,  
388.
- Obstetrical Society, Proceedings of the,  
75, 174, 281, 343, 464, 560.
- Œdema glottidis, 487.
- Œsophagismus, Dr. A. W. Foot on, 327.



- Onychia maligna, nitrate of lead in, 145.
- Optic nerve in cases of cerebral tumours, 538.
- Oral surgery, Garretson's, *Rev.*, 415.
- Osteitis. Dr. Barton's case of, 96.
- Ovarian cysts, tapping of, 529—disease, treatment of, 527—tumour, diagnosis of, 523—tumours complicating pregnancy, 533.
- Ovariectomy, 515—use of drainage-tube after, 536.
- Ovary, cystic disease of, 94—colloid carcinoma of, 370.
- Oxygen and ozone; their therapeutic uses, 139.
- Ozone and oxygen, 139.
- Patella, comminuted fracture of, 207.
- Pathological charts, *Rev.*, 48—Society of Dublin, Proceedings of the, 92, 205, 296, 370, 475.
- Peebles, Dr. W. B., on a case of dislocation of the kidney, renal abscess, and recovery, 348.
- Penis, amputation of, at pubes, 222.
- Perchloride of iron in *post-partum* hæmorrhage, 346 *seq.*, 465 *seq.*—injection of hæmorrhoids with, 505.
- Perforation in enteric fever, 297.
- Peritonitis without perforation in enteric fever, 301.
- Phosphorus, administration of, 139.
- Photographic clinique of the British Hospital for Diseases of the Skin, *Rev.*, 135.
- Phthisis, laryngeal, 475—pneumonic and intestinal, 485.
- Pirrie, W., work on surgery, *Rev.*, 411.
- Placenta, retained, 189.
- Plaster-splint, Dr. P. H. Watson's, 98.
- Pleura, hæmorrhagic sarcoma of, Dr. Gordon's case of, 452.
- Pneumatic aspiration, Dieulafoy's treatise on, *Rev.*, 440.
- Pneumonic and intestinal phthisis, 485.
- Porter, Mr. F. T., on a case of hysterical convulsions, 287.
- Porter, Mr. G. H., case of cystic disease of the ovary, 94—case of fracture of os calcis, 94.
- Post-partum* hæmorrhage, preventive treatment of, 75.
- Potassic bromide, in epilepsy, 163.
- Practical observations, by Mr. Fitzmaurice, 13.
- Pregnancy complicated with an ovarian tumour, 533.
- Pregnancy, mole, 281.
- Preventive treatment of *post-partum* hæmorrhage, 75.
- Prince, galvano-therapeutics, *Rev.*, 445.
- Proceedings of the Pathological Society, 92, 205, 296, 370, 475.—Obstetrical Society, 75, 174, 281, 343, 560.
- Prolapse of funis, 190.
- Propylamine, 64.
- Puerperal convulsions, 560.
- Purdon, Dr. H. S., on lupus erythematosus, 388—delirium tremens, 394—lichenoid eruption on flax-spinners, 507.
- Quinlan, Dr., a case of carcinoma of liver, 303.
- Recent works on diseases of the urinary organs. By Sir Henry Thompson, *Rev.*, 56.
- Remarks on small-pox at Tralee, 13.
- Renal disease, chronic, 478.
- Report on small-pox at Mullingar, 6—twenty-second, of the district, &c., asylums in Ireland, 1872, *Rev.*, 44—on materia medica and therapeutics, by W. G. Smith, M.D., 136—of the Rotunda Lying-in Hospital, 177—on midwifery, by G. H. Kidd, M.D., 515.
- Reports, select clinical. By Dr. Foot, 209.
- Rest; a therapeutic agent, by Dr. Finny, 108.
- Reynolds, J. Russell, on electricity, *Rev.*, 445.
- Richardson's simplicity of life, *Rev.*, 420.
- Ringland, Dr. A. H., on *post-partum* hæmorrhage treated by the application of the solid perchloride of iron to the interior of the uterus, 356.
- Roberts, Dr. F. T., handbook of the theory and practice of medicine, *Rev.*, 326.
- Rockwell, A. D., on electro-surgery, *Rev.*, 445.
- Roosa's disease, of the ear, *Rev.*, 435.
- Rotunda Lying-in Hospital, report on, 177.
- Rubeola, 193.
- Rumbold, on the function of the Eustachian tube, *Rev.*, 406.
- Sarcoma of pleura, hæmorrhagic, Dr. Gordon's case of, 452—of testicle, 481.
- Schiller's "Don Carlos," Wood's translation of, *Rev.*, 432.
- Seguin on family thermometry, *Rev.*, 407.
- Select clinical reports, by Dr. A. W. Foot, 209.
- Senna, effects of, on the urine, 138.
- Simplicity of life, Richardson's, *Rev.*, 420.
- Skin, works on diseases of the, *Rev.*, 134.
- Skull, fractures of the, Dr. Corley on operative interference in, 305.
- Small-pox, report on, at Mullingar, 6—remarks on, at Tralee, 13.
- Smith, Dr. A. H., on the Caisson Disease, *Rev.*, 448.

- Smith, Dr. W. G., on therapeutical remedies recently introduced, 57—report on materia medica and therapeutics, 136—case of congenital excess of ureters, 384.
- Society, Transactions of the Medical, 57, 151, 266, 327, 452, 538—Ulster Medical, 385—Proceedings of the Dublin Obstetrical, 75, 174, 281, 343, 464, 560—Proceedings of the Pathological, of Dublin, 92, 205, 296, 370, 475.
- Spasmodic closure of the cesophagus, Dr. A. W. Foot on, 327.
- Stauungs papilla, 542.
- Stoker, Dr. Thornley, on cicatrix of hand from a burn, 302.
- Stokes, Dr., on enteric fever, 483—lectures on fever, *Rev.*, 510.
- Stokes, Professor, jun., case of disease of knee-joint, 376.
- Stomach, ulcer of, 390.
- Stricture of urethra, Professor Bennett's case of, 378.
- Surgery, principles and practice of, Pirrie, *Rev.*, 411.
- Surgical anatomy, Bellamy's guide to, *Rev.*, 442—inquiries, Jordan's, *Rev.*, 443.
- Syphilitic osteitis and periostitis, by J. Hamilton, *Rev.*, 315.
- Tapping of ovarian cysts, 529.
- Tasmania, climate and vital statistics of, 151.
- Tetanus following abortion, by Dr. Boyd, 583.
- Therapeutical remedies recently introduced, by Dr. W. G. Smith, 57.
- Thermometry, family, by Dr. Seguin, *Rev.*, 407.
- Thompson, Sir Henry, on diseases of the urinary organs, *Rev.*, 56.
- Thomson, Dr. W., excision of the knee, 486.
- Tibbits, Dr. R., medical electricity, *Rev.*, 447.
- Tongue, Dr. Fairlie Clarke's treatise on diseases of the, *Rev.*, 317.
- Transactions of the Medical Society of the College of Physicians, 57, 151, 266, 327, 452, 538—Ulster Medical Society, 385.
- Treatment of small-pox, 11, 14.
- Tricuspid and mitral stenosis, coincident, Dr. Hayden on, 459.
- Tufnell, Mr., case of abdominal aneurism, 299.
- Turner, Prof., journal of anatomy and physiology, *Rev.*, 407.
- Typhoid fever; its nature, mode of spreading, and prevention, by Dr. Budd, *Rev.*, 311.
- Tyrrell, Mr. H. J., on excision of the knee-joint and its after-treatment, 97—amputation of the penis at the pubes, 222—sarcoma testis, 481.
- Ulceration of the cartilages of knee-joint, Professor Stokes' case of, 376—stomach, 390.
- Ulster Medical Society, Transactions of the, 385.
- Ureters, congenital excess of, 384.
- Urethra, rupture of, in labour, 290—stricture of, 378.
- Urinary organs, diseases of the, *Rev.*, 56.
- Urine, effects of senna on the, 138.
- Uterine tumours, Dr. Atthill on, 174, 381—fibrous tumour, Dr. Kidd's case of, 281—hydatids, 281, 464—malignant tumours, 381.
- Version, 186.
- Vital statistics of Tasmania, 151.
- Walshe, Dr. W. H., a practical treatise on the diseases of the heart and great vessels, *Rev.*, 130.
- Watson's, Dr. P. H., plastic-splint, 97, 98.
- West, on diseases of children, *Rev.*, 408.
- Wharton, Dr., a case of laceration of lungs and heart, 300.
- Whitla, Dr., case of horse-shoe kidney, 391.
- Wilson, Erasmus, on dermatology, *Rev.*, 414.
- Wood's Schiller's "Don Carlos," *Rev.*, 432.
- Works on diseases of the skin, *Rev.*, 134, 447—the Eustachian tube, *Rev.*, 406—electricity, *Rev.*, 444.
- Xylol, 61.
- Yule, on the mechanism of opening and closing the Eustachian tube, *Rev.*, 406.
- Zymotic disease and carbolic acid, 138—deaths, 200.



# PERIODICALS WITH WHICH THE DUBLIN JOURNAL IS EXCHANGED.

## GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. Pharmaceutical Journal. Churchill.
5. The Lancet.
6. The British Medical Journal.
7. The Asylum Journal of Mental Science. Churchill.
8. The Glasgow Medical Journal. Dunn and Wright.
9. The Dublin Medical Press and Circular.
10. The Westminster Review. Trübner.
11. Transactions of Obstetrical Society. London : Longmans.
12. The Practitioner ; a Monthly Journal of Therapeutics. Macmillan and Co.
13. The Journal of Anatomy and Physiology. Macmillan.
14. The British Journal of Homœopathy. London : Henry Turner and Co.
15. Irish Hospital Gazette. Dublin.
16. The Obstetrical Journal. London : J. and A. Churchill.

## INDIA.

17. Indian Medical Gazette. Calcutta : G. Wyman and Co.

## AUSTRALIA.

18. The Australian Medical Journal, Melbourne : Stillwell and Knight. London : H. Baillière.

## AMERICA.

19. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia : Henry C. Lea. London : Trübner and Co.
20. The American Journal of Science and Arts. Conducted by Professors B. Silliman, and J. D. Dana, &c. New Haven : Editors.
21. The American Journal of Insanity, Utica, N. Y. State Lunatic Asylum.
22. The American Journal of Obstetrics and Diseases of Women and Children, New York : W. A. Townsend and Adams. London : S. Low, Son, and Marston.

## AMERICA.—continued.

23. Canada Medical Journal. Montreal : Dawson, Brothers.
24. The New York Medical Journal. New York and London : D. Appleton and Co.
25. The Medical and Surgical Reporter. Philadelphia : S. W. Butler, M.D.
26. The Medical Record. New York : Wood & Co.
27. The American Practitioner. Louisville, Ky. : John P. Morton and Co. London : C. D. Cazenove.
28. The Philadelphia Medical Times. Philadelphia : J. B. Lippincott and Co.
29. The Sanitarian. A. S. Barnes and Co., 111, William-street, New York.
30. The American Chemist, School of Mines, Columbia College, East Forty-ninth-street, New York.

## FRANCE.

31. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue des nouvelles scientifiques, nationales et étrangères, &c. Paris : Labbé.
32. Gazette Médicale de Paris. Paris : 4, Place Saint-Michel.
33. Journal de Pharmacie et de Chimie, &c. Paris : Victor Masson.
34. L'Union Médicale. Paris : Bureau, Rue de la Grange-Batelière.
35. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
36. Archives Générales de Médecine. Paris : Asselin.
37. Bulletin de l'Académie de Médecine. Paris : G. Masson.
38. Revue de Thérapeutique Médico-Chirurgicale. Paris : Dr. A. Martin-Lauzer.
39. Journal de Médecine et de Chirurgie Pratiques à l'Usage des Médecins. Par Lucas-Championnière. Paris.
40. Journal des Connaissances Médicales Pratiques. Paris : J. B. Baillière et Fils.
41. Annales Médico - Psychologiques. Par MM. Baillarger, Cerise, et Lunier. Paris : V. Masson.
42. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Par le Docteur Félix Bricheateau. Paris.
43. Répertoire de Pharmacie. Par M. Eug. Lebaigue. Paris : Rue de la Perle, 11.
44. Gazette Médicale de Strasbourg.



## FRANCE.—continued.

45. *Annales de Gynécologie*. Paris : H. Lauwereyns.  
 46. *Gazette des Hopitaux*. Paris : 57, Rue des Saints-Pères.  
 47. *Lyon Médical, Organe Officiel de la Société Impériale de Médecine*. Lyon : Mégret.  
 48. *Journal de Médecine Mentale*. Paris : M. Delasiauve. Paris : Masson et Fils.  
 49. *Archives de Médecine Navale*. Paris : J. B. Baillière et Fils.  
 50. *Revue Photographique des Hôpitaux de Paris*. Paris : Adrien Delahaye.  
 51. *Le Mouvement Médicale*. Paris : Rue des Ecoles.  
 52. *La France Médicale*. 21 Rue de la Monnaie, Paris.  
 53. *Revue des Sciences Médicales en France et à l'étranger*. Paris : G. Masson.  
 54. *Le Progres Médical*. Paris : E. Duval.  
 55. *Gazette Hebdomadaire*. Paris : 91, Rue de Lille.

## BELGIUM.

56. *Bulletin de l'Académie Royale de Médecine de Belgique*, Bruxelles.  
 57. *Annales D'Oculistique*. Bruxelles.  
 58. *Annales et Bulletin de la Société de Médecine de Gand*.

## GERMANY.

59. *Vierteljahrsschrift für die praktische Heilkunde*, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.  
 60. *Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern*. Redigirt von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.  
 61. *Wochenblatt der Zeitschrift der k. Gesellschaft der Aerzte in Wien* (Beilage zu den Jahrbüchern). Redigirt von A. Duchek, C. Langer, A. Schauenstein. Leipzig: Hinrichs.  
 62. *Deutsches Archiv. für Klinische Medicin*. Erlangen: Th. Blaesing.  
 63. *Jahrbuch für Kinderheilkunde und Physische Erziehung*. Leipzig: B. G. Teubner.

## PRUSSIA.

64. *Archiv für pathologische Anatomie und Physiologie, &c.* Herausgegeben von R. Virchow. Berlin: G. Reimer.  
 65. *Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medicin*

## PRUSSIA.—continued.

- Herausgegeben von Damerow, Flemming, Roller; durch Heinrich Laehr. Berlin : Hirschwald.  
 66. *Berliner Klinische Wochenschrift*. Berlin : Hirschwald.  
 67. *Archiv für Klinische Chirurgie* Herausgegeben von Dr. B. von Langenbeck. Berlin : Hirschwald.  
 68. *Monatsschrift für Geburtskunde und Frauenkrankheiten*. Berlin : Hirschwald.  
 69. *Archiv für Psychiatrie und Nerven Krankheiten*. Berlin : August Hirschwald.  
 70. *Centralblatt für die medicinischen Wissenschaften*. Berlin : August Hirschwald.

## HOLLAND.

71. *Archiv für die Holländischen Beiträge zur Natur- und Heilkunde*, Herausgegeben von F. C. Donders, Utrecht, und W. Berlin, Amsterdam, Utrecht: C. Van Der Post.

## NORWAY.

72. *Norsk Magazin for Lægevidenskaben*. Udgivet af det medicinske Selskab i Christiania. Redigeret af Schoenburg. E. Winge. Bidenkap. Christiania: Paa Th. Steens Forlag.

## SWEDEN.

73. *Hygiea, Medicinsk och Farmaceutisk Maonads-skrift*. Stockholm .P. A. Norstedt och Söners förlag.  
 74. *Nordiskt Medicinskt Arkiv*. Redigeradt af Dr. Axel Key, Prof. i Patolog. Anat. i Stockholm. Stockholm: Samson och Wallin.  
 75. *Upsala Lakäreförenings Forhandlingar*. Upsala: Ed. Berling.

## DENMARK.

76. *Hospitals-Tidende*. Optegnelser af praktisk Lægekunst fra Ind- og Udlandet. Copenhagen: Jacob Lund. London : Asher and Co.

## ITALY.

77. *Bulletino delle Scienze Mediche*. Pubblicato per cura della Società Medico-Chirurgica di Bologna.  
 78. *Giornale Veneto di Scienze Mediche*.  
 79. *Lo Sperimentale Giornale Critico di Medicina e Chirurgia per servire ai Bisogni dell'Arte Salutare*. Direttore Prof. C. C. M. Butalini. Florence.

